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THE
CONNEXION OF LIFE
WITH
RESPIRATION;
OR, AN
EXPERIMENTAL INQUIRY
INTO THE EFFECTS OF
SUBMERSION, STRANGULATION,
AND
SEVERAL KINDS OF NOXIOUS AIRS,
ON
LIVING ANIMALS:
WITH
An ACCOUNT of the NATURE of the DISEASE they produce; its Diſtinction from Death itſelf; and the moſt effectual MEANS of CURE.

By *EDMUND GOODWYN*, M.D.

> *Arteria animam accipit è pulmonibus.* CICERO.

LONDON:
Printed by T. SPILSBURY, Snow-hill,
For J. JOHNSON, in St. Paul's Church-yard.
1788.

Sanguineum laticem, turgens quem vena reportat,
Atque in pulmonum ſubito penetralia vibrat,
Imbibiturum illic, jam quidquid ab aëre ſumpto
Ætheris expreſſum eſt:——————
——————Motu, proh! quantum eſt artis in illo!
Vivimus hac fabrica tantum, ceſſante perimus:
Machina nam noſtra hæc non eſt hydraulica ſolum,
Pneumatica eſt etiam; auxilio ſpirabilis auræ
Indiget.——————

Polignac, *Anti-Lucretius.*

TO

Mr. *JOHN PAGE*, ſen^{r}.

OF

WOODBRIDGE, SUFFOLK,

THIS ESSAY

IS INSCRIBED,

AS A MARK OF GRATITUDE AND ESTEEM,

BY

THE AUTHOR.

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INTRODUCTION.

THE outlines of this Eſſay were publiſhed, about two years ſince, in the Latin language; and ſeveral of the moſt eminent profeſſional men honoured them with particular attention. A reception ſo flattering from intelligent criticiſm gave me an unexpected importance, and encouraged me to continue my labours with the hope of preſenting them to the publick in a more perfect form.

In the courſe of theſe labours many difficulties have ariſen, and much exertion has been employed; and ſucceſs has often favoured me: ſome queſtions however are ſtill left undetermined, and ſome facts unexplained; but about theſe I am leſs anxious, as they are not immediately connected with the principal object of this Inquiry: and, although ſome ſmall obſcurities may ſtill remain, ſeveral facts are aſcertained, which will, I truſt, be of conſiderable uſe both in Philoſophy and Phyſick.

The reſpiration of animals has long been a difficult problem in Phyſiology, and no ſatisfactory ſolution could be given of it, whilſt Chymiſtry continued in an uncultivated ſtate: the general facts were indeed attended to, and conjectures propoſed to explain them (*a*); but as long as conjecture *only*

(*a*) Sir Iſaac Newton imagined that the atmoſpheric air might communicate an acid vapour to the blood in the lungs, which was neceſſary to keep up the action of the heart. OPTICS, p. 355.

only was uſed, doubt and uncertainty remained.

Within theſe few years the knowledge of chymiſtry has been much improved, and experiments have been applied to reſpiration with conſiderable effect; new facts have been diſcovered, extraordinary phænomena explained, and conſiderable obſcurities removed; but ſome difficulties ſtill remained to tempt the hopes of future labourers. In the courſe of this Inquiry I have carefully examined the facts deſcribed by others, and inſtituted a variety of new experiments, which ſeem to have aſcertained a principal uſe of reſpiration, its connexion with the motion of the heart and the other functions of the body.

The life of animals has alſo been a ſubject of attention at different times: for until we are acquainted with ſome of its eſſential qualities, we cannot expect to diſtinguiſh it from death. The ancient phyſicians ſeem to have been aware of this; but they did not labour to ſupply the defect.

About the middle of this century two writers of diſtinguiſhed character entered upon the inquiry (*a*), and purſued it with great ardour and enthuſiaſm for a conſiderable length of time: in this purſuit they diſplayed much learning and ingenuity, and collected many valuable facts from the writings of others; but ſtill their concluſions were unſatisfactory, and their opinions are now fallen into neglect. This indeed will not be wondered at by ſuch as have examined their works; for it is generally obſerved

---

(*a*) Whytt and Haller.

obſerved of them, that they often blend hypotheſis with fact, and ſometimes graſp at more than the mind can attain.

After this time the ſtudy of life was generally neglected: however, ſome profeſſional men attempted to ſearch for the diſtinction between life and death in the ſenſible qualities of the body. Obſervations were collected, and ſeveral marks propoſed to the world to determine this diſtinction (*a*): but ſubſequent obſervation has ſhown them to be inſufficient; and, after much reſearch and altercation, it is now generally allowed, that we have hitherto no other means to determine

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(*a*) Lettres ſur la Certitude des Signes de la Mort, par Monſ. Louis.

Diſſertatio an Mortis incerta ſigna minus à chirurgicis quam ab aliis experimentis. WINSLOW.

determine on abſolute death, but the preſence of putrefaction (*a*).

To avoid the ſources of theſe failures, I have endeavoured to conſider the ſubject in a different manner (*b*), taking the living body when all the accidental ſigns of life are removed, applying to it thoſe external powers which really do reſtore them, then attending to the place and circumſtances of their firſt operation, and the immediate effect they produce: thus I was led to the eſſential quality of life, and conſequently to the means of diſtinguiſhing it from death itſelf.

This

(*a*) Diſſertation ſur l'Incertitude des Signes de la Mort, par Monſ. Bruhier.

(*b*) Diſſertatio Inauguralis de Vita Corporis humani. Auctore J. T. Vander Kemp.

This diſtinction will be the more valuable, as it applies to every ſimilar ſtate of the body, whatever may have been the original diſeaſe; and if it be properly attended to, we may always determine with certainty, whether a perſon be really alive or dead; and the bodies of our friends may now be conſigned to the grave, before they become noxious to others.

But the benefits of this Inquiry have extended ſtill further.

From the reſult of the whole, I have been enabled to determine the nature of the diſeaſe that takes place from drowning, hanging, and breathing ſeveral noxious airs; to give it a deſcriptive name, and its proper place in the ſyſtems of noſology; and to point out the moſt effectual means of cure. Many remedies are recommended for this purpoſe in the directions of publick Societies of different countries, and all of them are ſaid to have been employed with ſucceſs; and ſince the aſſertion is favoured by ſome of the moſt reſpectable authors, both ancient and modern, no one can deny it without incurring the charge of preſumption: but if it ſhould appear here, that animals may be recovered from the diſeaſe with equal eaſe, without the concurring operation of a great many of them, a regard to truth, as well as publick utility, ought to determine our choice. Beſides, when it is remembered that a prepoſſeſſion in favour of theſe remedies may occaſion a conſiderable loſs of time, and that a loſs of time in this diſeaſe is often a loſs of life, the intereſt of humanity ſhould riſe above general prejudice, and reſpect for authority give way to deliberate inveſtigation.

In the whole of this Inquiry, I have endeavoured to adhere cloſely to the method of *Analyſis*, beginning with effects, and aſcending to their particular cauſes, and from particular cauſes to more general ones, with the aſſiſtance of *Analogy* and *Induction*, until I could proceed no further. Then I aſſumed the laſt concluſion as a general principle to explain the production of all the effects in a deſcending ſeries to the firſt phænomena, where the analyſis began; in this manner adding the *Syntheſis*, and thus giving to the reſult all the evidence which the nature of the ſubject would admit.

To avoid the danger of being miſled by the colouring of fancy or inaccurate obſervation, the experiments were repeated ſeveral times with great care and attention, and always in the preſence of ſome judicious friends: but as the teſtimony of the ſenſes is not always ſufficient, and the moſt careful inquirer may ſometimes err, I neither expect, nor even wiſh the concluſions ſhould be adopted, until the experiments ſhall have been repeated by others; and if it be afterward proved, that I have been miſtaken in any part of them, I ſhall readily retract the aſſertion, and acknowledge the error: for the intereſt of truth and the welfare of mankind are of more value than the reputation of an individual; and, as M. Bonnet properly ſays, "*Un* J'AI TORT *vaut mieux que cent répliques ingénieuſes.*"

LONDON,
May 6, 1788.

SECTION I.

To aſcertain the general Effects of Submerſion on living Animals.

IN the courſe of this inquiry, we propoſe to aſcertain the general effects of ſubmerſion on living animals, and to trace their connexion with the action of the water on the body.

The firſt of theſe objects may be obtained by attending to the different changes that take place in living animals while they are immerſed in water, and examining the appearances of the internal parts when the body is taken out; and the ſecond, by obſerving the manner in which the water is applied to the body in ſubmerſion, and tracing its particular operation in producing theſe changes.

For the firſt purpoſe, I procured a large tranſparent glaſs bell, that would allow me to diſtinguiſh accurately the circumſtances that took place within it; and when it was inverted, and filled with water, I put into it, at different times, ſeveral cats, dogs, rabbits, and other ſmaller animals, and confined them there till they had the appearance of being dead. As ſoon as they were put under the bell, I attended to the changes that took place in the body; and when they had loſt the external ſigns of life, I opened the head, the breaſt, and belly, and examined the internal parts.

Whilſt I was engaged in this buſineſs, the bodies of three unfortunate perſons, who had been drowned, were brought to me, at different times, with permiſſion to inſpect the internal parts: theſe I examined with great care and attention, and compared the appearances with what I had ſeen in the other animals.

In theſe different experiments, I obſerved ſome variations in the external morbid ſymptoms, and in the appearance of the internal parts; but the order and ſucceſſion of the ſymptoms, and the condition of the organs immediately connected with life, agreed uniformly with the following deſcription.

When an animal is immerſed in water, his pulſe becomes weak and frequent; he feels an anxiety about his breaſt, and ſtruggles to relieve it: in theſe ſtruggles he riſes towards the ſurface of the water, and throws out a quantity of air from his lungs. After this, his anxiety increaſes; his pulſe becomes weaker; the ſtruggles are renewed with more violence; he riſes towards the ſurface again, throws out more air from his lungs, and makes ſeveral efforts to inſpire; and in ſome of theſe efforts a quantity of water commonly paſſes into his mouth. His ſkin then becomes blue, particularly about the face and lips; his pulſe gradually ceaſes; the ſphincters are relaxed; he falls down without ſenſation, and without motion.

If the body be immediately opened, it has theſe appearances:

1°, The external ſurface of the brain is of a darker colour than uſual; but the veſſels are not turgid with blood, nor are there any marks of extravaſation about them.

2°, The cavity of the lungs contains a conſiderable quantity of frothy fluid; and the pulmonary arteries and veins are filled with black blood throughout their whole extent.

3°, The right auricle and ventricle of the heart are ſtill contracting and dilating; the left ſinus venoſus and auricle move feebly; but the left ventricle is at reſt.

4°, The right auricle and ventricle are filled with black blood, and the left ſinus venoſus and left auricle alſo; but the left ventricle is only about half filled with the ſame-coloured blood.

5°, The trunks and ſmaller branches of the arteries proceeding from the left ventricle, contain a quantity of this black blood alſo.

In the early ſtate of medical knowledge, and before anatomy was much cultivated, it was generally ſuppoſed, by different writers, that the water produced theſe effects on animals by ruſhing into all the cavities of the body, and rupturing ſome of the organs immediately connected with life (*a*). But ſince the ſtructure of the body has been more accurately examined, and the uſe and connexion of the different functions better underſtood, it appears evident that theſe changes are to be attributed entirely to the effects which the water produces on the lungs; either *directly*, by entering into their cavity; or *indirectly*, by preventing

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(*a*) Galen. Lib. III. Comment. 40.—Ægineta, Lib. III. p. m. 97.—Ætius, Art. Princip. p. 404.—Alexand. Bened. Cap. III.—Codrongus de Submerſis, p. m. 322.—Senertus in Praxi, Lib. II. p. 350.

preventing the entrance of the air of the atmoſphere.

Several authors have endeavoured to diſcover by which of theſe means the water operates; and, after conſiderable labour and attention, they were greatly divided in their opinions: ſome contended for the former (*a*), and others for the latter (*b*). The concluſions, however, of both parties were generally drawn either from caſual obſervation, or from inſufficient and undigeſted experiments;

(*a*) *Platner*. Cent. Quæſt. Paradox. p. 35.—*Louis*, Mémoires ſur les Noyés, *&c. &c.*—*De Haen*, Ratio Medendi continuata, tom. 1, *&c.*

(*b*) *Littreus* and *Senac*.—Hiſtoire de l'Académie Royale des Sciences, anno 1719.—*Haller*, Prælect. Boerhaav. tom. 2. p. 219.—*Winſlow*, Experimenta Bruherii.—*Kaaw Boerhaave*, Impetum faciens, p. 228, *&c. &c.*

ments; in conſequence of which neither of the opinions has been eſtabliſhed by its partizans; and it ſtill remains a queſtion in phyſiology, which we ſhall next attempt to decide by experiment.

SECTION II.

To determine whether the Water produces theſe Changes directly, *by entering into the Cavity of the Lungs; or* indirectly, *by excluding the atmoſpheric Air.*

IF the water enter into the lungs in drowning, we may naturally expect to find it there afterwards, when the body is opened: but if the body be opened after the obſtruction of reſpiration from any other cauſe, there is often a frothy fluid in the cavity of the lungs, reſembling a mixture of air and water, which might miſlead the inquirer.

In conſequence of this, it has been ſuggeſted to add ſome colouring ſubſtance to the water employed in experiments of this kind, that, if it ſhould enter into the lungs in the proceſs of drowning, the peculiarity of colour might ſerve to diſtinguiſh it from this frothy fluid: but if the coloured fluid ſhould enter into the lungs in a ſmall quantity only, the colouring matter may be ſo much diluted by the pulmonary mucus, as to loſe in a great meaſure its diſtinguiſhing character. On this account, it will be better to employ the deepeſt colours in theſe experiments, ſuch as very black ink, ſtrong ſolutions of blue vitriol, &c.

EXPERIMENT I.

I filled my glaſs bell with ink, and immerſed a dog in it. As ſoon as his ſtruggles ceaſed, he was taken out, and examined. A ſmall quantity of frothy fluid was found in the lungs, and it was coloured with ink.

The ſame experiment was made on three other dogs; and the fluid found in the lungs of each of them was coloured with ink. It was alſo repeated on three cats, and with the ſame general reſult.

From theſe experiments it appears, that ſome of the ink paſſed into the lungs of theſe animals; and we may infer from it, that a quantity of water commonly paſſes into the lungs of animals that are drowned: but it may be ſtill ſuſpected, that this water enters into the lungs by its own gravity, after the ſtruggles of the animals have ceaſed; and therefore that the preſence of water in the lungs cannot have occaſioned the ſymptoms which preceded.

This queſtion may be decided by putting the bodies of animals into a ſtate ſimilar to that which ariſes from ſubmerſion; and, after the ſtruggles have ceaſed, by plunging them into a coloured fluid: for if the fluid enters into the lungs in this ſtate, it may be inferred, that the ink alſo entered into them by its own gravity, after the efforts to inſpire ceaſed.

EXPERIMENT II.

I paſſed a ligature round the trachea of a dog, and ſtrangled him. As ſoon as he ceaſed to move, I plunged the body into a quantity of ink, and confined it there for ſeveral minutes. On examining the lungs afterwards, I found no ink in them.

This experiment was repeated on two other dogs, and with the ſame reſult.

Here we find, that the ink does not enter into the lungs by its own gravity, after the efforts to inſpire ceaſe. It muſt therefore have entered into the lungs of the animals of the firſt experiment during the efforts to inſpire.

But if the ink entered into the lungs of theſe animals during the efforts to inſpire, was it not ſufficient to produce the changes that took place in the body in conſequence of ſubmerſion?

To determine this queſtion, we muſt aſcertain the quantity of water that paſſes into the lungs during ſubmerſion, and the changes that are produced on the body by the introduction of an equal quantity of a ſimilar fluid.

If the ink which paſſes into the lungs would remain there in a ſeparate ſtate, we might aſcertain the quantity that paſſed into them in theſe experiments, by preſſing it out again at the trachea: but this ink is ſo intimately combined with the mucus of the lungs by the agitation of the thorax, that we cannot collect it in a ſeparate ſtate. If, however, we employ ſuch fluids in theſe experiments as have no diſpoſition to unite with the pulmonary mucus, they will remain in the lungs in a ſeparate ſtate, notwithſtanding the agitation of the thorax, and may be in a great meaſure collected again.

EXPERIMENT III.

I filled a ſmall glaſs bell with quickſilver, and immerſed a cat in it, in the ſame manner as in the other experiments. When the body was taken out, I found half an ounce of quickſilver (*a*) in the cavity

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(*a*) In weight.

cavity of the lungs, and an ounce of a frothy fluid (*a*) tinged with a red colour.

Three more cats were immerſed in quickſilver, in the ſame manner; and when they were removed, we obtained from the lungs of

	Quickſilver.	*Frothy mucus.*
The firſt, -	three drachms,	ſix drachms:
The ſecond,	five drachms,	an ounce:
The third,	———	an ounce.

Four rabbits were drowned in quickſilver, in the ſame manner; and when they were removed, we obtained from the lungs of

	Quickſilver.	*Frothy mucus.*
The firſt, -	two drachms,	ſix drachms:
The ſecond,	one drachm,	half an ounce:

And from the two laſt we got no quickſilver.

Hence

(*a*) By meaſure.

Hence it is evident, that the whole quantity of the fluid found in the lungs of drowned animals is inconſiderable, and that it is generally compoſed partly of the natural mucus of the lungs, and partly of the fluid which paſſes into them during the efforts to inſpire.

Since, then, the quantity of the fluid which enters into the lungs is ſo very inconſiderable, are we to ſuppoſe that it alone produces the changes which take place in conſequence of ſubmerſion?

If the quickſilver alone produced theſe changes in the animals of the laſt experiment, the ſame changes will be produced by introducing an equal quantity of any fluid into the lungs of a living animal, when it is not immerſed in water.

It was ſhown, in the laſt experiments, that the greateſt quantity of quickſilver which paſſed into the lungs of the cats, was five drachms. Let us ſuppoſe a quantity equal to the whole of the mucus and quickſilver obtained from the lungs, paſſed into them from without: if it occaſioned the death of the animal, we ſhall produce the ſame effect by introducing an equal quantity of water into the lungs of a ſimilar animal, whilſt the reſpiration is not otherwiſe interrupted.

EXPERIMENT IV.

I confined a cat in an erect poſture, and made a ſmall opening in the trachea by cutting out one of the cartilaginous rings. Through this opening I introduced two ounces of water into the lungs. The animal had immediately a difficulty of breathing, and a feeble pulſe: but theſe ſymptoms were ſoon abated; and it lived ſeveral hours afterwards without much apparent inconvenience. At length I ſtrangled it, and found two ounces and a half of water in the lungs.

I introduced two ounces of water into the lungs of two other cats, in the ſame manner. The difficulty of breathing, and alteration of pulſe, were ſomewhat greater than in the firſt experiment; but in a few hours theſe complaints were abated: the animals were then ſtrangled, and four ounces of fluid were found in the lungs.

Here then, although a quantity of water be introduced into the lungs, greater than the whole quantity found in them in theſe experiments, yet it does not produce in the body thoſe changes that take place in drowning.

It follows, therefore, that the water which enters into the lungs of animals in drowning, is not the cauſe of the changes that take place in the body.

From theſe experiments we draw the following concluſions:

1°, A ſmall quantity of water commonly paſſes into the lungs in drowning.

2°, The water enters into the lungs during the efforts to inſpire, and, mixing with the pulmonary mucus, occaſions the frothy appearance mentioned by authors.

3°, The whole of this fluid in the lungs is not ſufficient to produce the changes that take place in drowning.

And hence it follows, *That the water produces all the changes that take place in drowning*, indirectly, *by excluding the atmoſpheric air from the lungs.*

To trace theſe changes to the excluſion of air, we muſt inquire into the particular effects of the air on the lungs in reſpiration, and the connexion of theſe effects with the different functions of the body: and when theſe are aſcertained, it will be eaſy to determine what changes muſt take place in conſequence of the privation of air.

The firſt and moſt obvious effect produced in the lungs by reſpiration, is an alteration in the volume of air they contain; and this muſt occaſion a proportional change in the degree of their dilatation, and conſequently in the form of the veſſels diſtributed upon them.

We proceed to inveſtigate theſe changes, and to trace their connexion with the other functions of the body.

SECTION III.

To determine the mechanical Effects of the Air on the Lungs in Reſpiration.

TO obtain the object propoſed in this Section, we muſt firſt aſcertain the different quantities of air in expiration and in inſpiration, and the proportional dilatation of the lungs in each of theſe ſtates; and then endeavour to determine the effects of theſe different degrees of dilatation on the pulmonary veſſels, and on the current of the blood circulating through them.

Several writers have attempted to meaſure the quantity of air taken in by a ſingle inſpiration; and ſome of them have delivered eſtimates of the proportional dilatation of the lungs, both in inſpiration and in expiration (*a*). Theſe eſtimates were in a great meaſure adopted by a celebrated phyſiologiſt; and he deduced many conſequences from them, to explain a variety of diſeaſes immediately connected with theſe mechanical changes (*b*): but the means employed to form theſe eſtimates appear to have been inſufficient; and the conſequences deduced from them are contradicted by ſeveral common appearances of the body: we ſhall therefore attempt to meaſure

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(*a*) *Borelli* de Motu Animal. Lib. II.—*Jurin*, Diſſert. IV. Lib. IV.—*Hales*, Veget. Statics, Vol. II. —*Sauvage* de Reſpiratione difficili.—*Bernoulli*, Diſſert. de Reſpiratione.

(*b*) *Haller*, Element. Phyſiolog.

meaſure theſe quantities again, and to form our eſtimate from the reſult of the experiments:

And, firſt, to meaſure the quantity of air taken into the lungs after a complete expiration.

As every animal makes a full expiration immediately before death, the lungs in a dead body are in a ſtate of complete expiration: if, therefore, we can meaſure the quantity of air in the lungs of dead bodies, we ſhall determine the medium quantity in a ſtate of complete expiration. Now, it is generally known, that the lungs, in a ſound ſtate, are always contiguous to the containing parts of the thorax; and that all the containing parts of the thorax, except the diaphragm, are fixed after death: if, then, we fix the diaphragm of a dead body, and make an opening through the external parts into the cavity of the thorax, the atmoſpheric air will paſs in by its own gravity, and, preſſing upon the external ſurface of the lungs, will force them to collapſe, and expel the air they contain; and that part of the cavity of the thorax which they before occupied, will be the meaſure of the volume of air expelled from them. If we now fill up this part of the cavity with water, it will give the quantity of air which was in the lungs after expiration.

EXPERIMENT I.

I procured a dead body of ordinary ſtature, and applied a cloſe compreſs upon the ſuperior part of the abdomen, to fix the diaphragm in its ſituation: then I made a ſmall opening into the cavity of the thorax on each ſide, and upon the moſt elevated part. The lungs immediately collapſed; and conſequently the air which they contained was expelled. Water was next introduced at theſe openings, till the cavity was filled; and the contents of the water were 272 cubic inches.

The lungs of this body contained therefore 272 cubic inches of air in a ſtate of complete expiration.

The ſame experiment was repeated on two other bodies, nearly in the ſame circumſtances; and the lungs of the firſt, in a ſtate of complete expiration, contained 250 cubic inches of air, and the lungs of the ſecond 262 cubic inches.

The ſubjects of theſe experiments died from hanging; and I was not aware that there could be any objection to the experiment

ment on that account: but it afterwards occurred to me, that their lungs were not in a ſtate of expiration; becauſe perſons under the influence of fear often make a deep inſpiration, before the cord is paſſed round the trachea; and the preſſure of the cord becomes immediately ſo tight, that they are not able to expel the air again.

On this account I repeated the experiment on ſeveral adult ſubjects, who died a natural death. In ſome of them the lungs adhered to the ſides of the thorax, and did not collapſe completely, when the openings were made; but in four of them the lungs appeared to collapſe properly; and we had the following reſults:

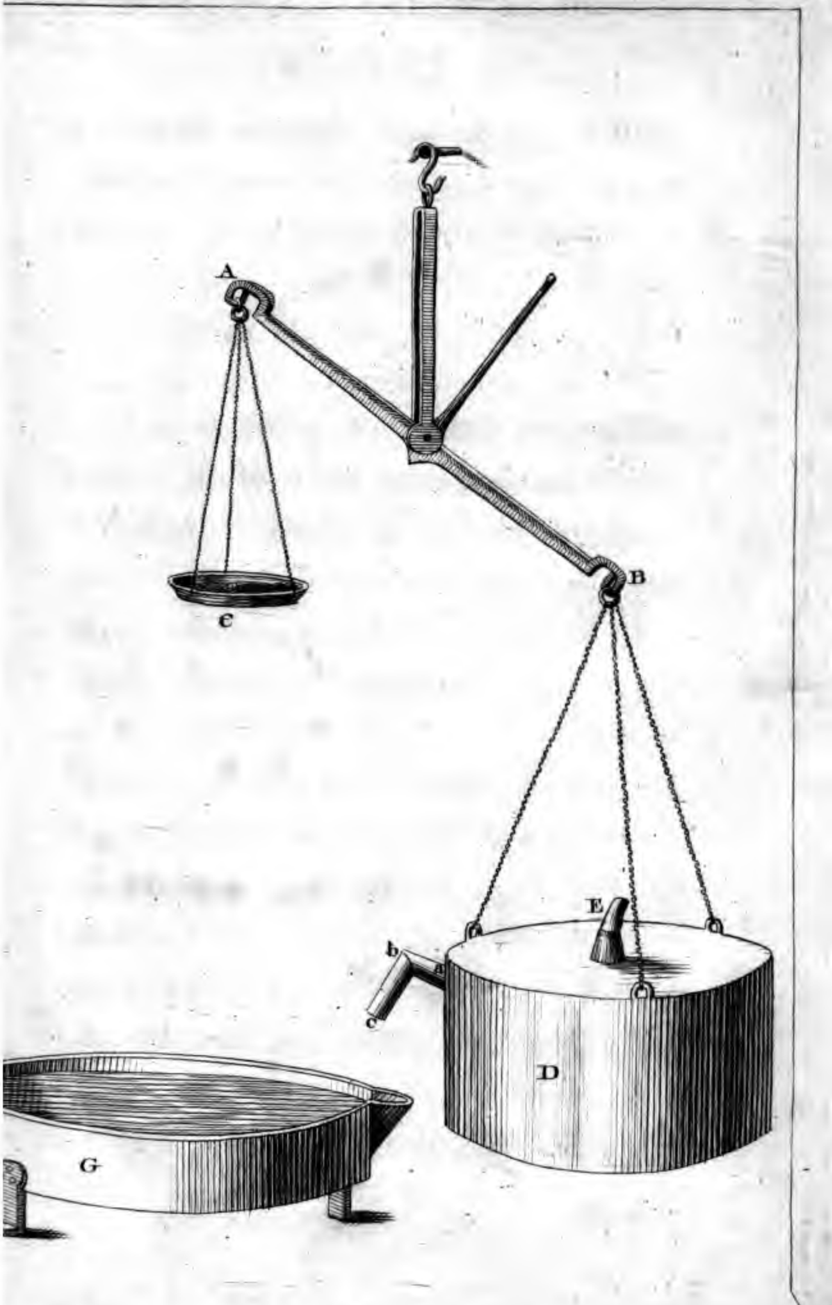
The lungs of the firſt contained	120 cubic inches,
———— the ſecond	102
———— the third	90
———— the fourth	125

Theſe experiments are ſufficient to ſhow, that the lungs contain a conſiderable quantity of air, even after complete expiration: but this quantity muſt vary in different ſubjects in proportion to the capacity of the thorax; it is therefore extremely difficult to eſtabliſh a medium. However, to purſue this inquiry, we ſhall for the preſent adopt the medium quantity of theſe latter experiments, and ſay that the lungs of the human ſubject contain 109 cubic inches of air after complete expiration.

We next proceed to meaſure the quantity of air taken into the lungs at an ordinary inſpiration.

This may be done by breathing from a veſſel which has only two tubular openings, one to be applied to the mouth, whilſt the other is immerſed in water. If we inſpire from the veſſel, under theſe circumſtances, a quantity of water will enter it, equal in volume to the quantity of air inſpired from it.

On this principle we contrived the machine A B C D E. The veſſel D contains ſeveral hundred cubic inches of air. This veſſel (which for the ſake of diſtinction I call pneumatic) ſuſpended in the balance A B, and brought to an equilibrium with the ſcale C; and the tube *a b c* being immerſed in water in the veſſel G; if a perſon inſpires from the tube E, a volume of water will enter the pneumatic veſſel through the tube *a b c*, equal to the volume of air inſpired from it by the tube E. The ſcale C will determine the weight of the water in the pneumatic veſſel; and this, by calculation, will give the number of cubic inches of water which entered the machine at


A
B
C
E
b
a
c
D
G

at a ſingle inſpiration, and the number of cubic inches of water will give the number of cubic inches of air.

EXPERIMENT II.

An adult perſon of ordinary ſtature inſpired twice from the machine; imitating, as nearly as he could, an ordinary inſpiration.

In the firſt he took in 3 cubic inches of air;
In the ſecond 2½ cubic inches.

Another perſon, of nearly the ſame ſtature, inſpired twice:

In the firſt he took in 3⅕ cubic inches of air;
In the ſecond 2¾ cubic inches.

In theſe experiments there is a difference in the quantity of air taken into the lungs at a ſingle inſpiration, which we ſuſpected might ariſe from the attention of the mind in this new ſituation of breathing. To avoid this ſource of error, as much as poſſible, we determined that the ſame perſon ſhould inſpire regularly from the pneumatic veſſel for a minute or two, and expire alternately into the atmoſphere, numerating all the inſpirations; and that we ſhould then meaſure the water which paſſed into the veſſel during that time, and calculate the average quantity for each inſpiration.

Experiment III.

The two perſons employed in the laſt experiment inſpired from the pneumatic veſſel thirty times ſucceſſively, in the manner juſt deſcribed; and the average quantity of air taken in at a ſingle inſpiration, by the firſt perſon, was $2\frac{3}{4}$ cubic inches;

By the ſecond, $3\frac{1}{6}$.

It appears, then, that the quantity of air taken into the lungs at a ſingle inſpiration is very inconſiderable, when compared with what they contain after a complete expiration. This extraordinary difference was remarked with ſurpriſe by ſeveral judicious friends; and they expreſſed their fears that I might have been deceived. In conſequence of this, we repeated the ſecond experiment again, with ſtill more attention, and on a greater variety of ſubjects; and in all of them the average quantity of each inſpiration was nearly the ſame as in the preceding: but in the third experiment an anxiety was felt in the breaſt, before the limited number of inſpirations was finiſhed; and when the mouth was withdrawn from the tube, we obſerved that it was neceſſary to make a deep inſpiration. Theſe two circumſtances ſeemed to ſhow, that the quantity of air taken into the lungs from the machine had not been ſufficient for the purpoſes of reſpiration; and this deficiency in quantity muſt be attributed to ſome particular circumſtances of the experiment.

In all theſe experiments we attended to the *effort* made by the organs of reſpiration when the ordinary quantity of air is taken in from the atmoſphere; and we conſidered the force of this *effort* as the meaſure of an ordinary inſpiration (*a*), and endeavoured to copy it exactly when we inſpired from the machine: but we find that this is not ſufficient; becauſe the water muſt riſe into the

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(*a*) Every one knows, that there is a conſiderable difference in the quantity of air taken into the lungs at an ordinary, and at a full inſpiration: if a perſon expires carefully, and then makes a full inſpiration, he will frequently take into his lungs upwards of 200 cubic inches of air at a ſingle effort.

the pneumatic veſſel contrary to its own gravity; and in order to overcome this additional reſiſtance, it will be neceſſary that the effort to inſpire from the machine be greater than the ordinary *effort* in the atmoſphere. And this has been the ſource of the miſtake.

Since, then, the effort in the atmoſphere is not the meaſure of a natural inſpiration from the machine, we muſt have recourſe to the ſenſation in the lungs. If we inſpire from the pneumatic veſſel ſeveral times ſucceſſively, as in the laſt experiment, and take in as much air at each time as to leave no uneaſineſs in the lungs during the inſpirations, nor any want of a greater quantity when the mouth is removed, we may conclude that the lungs have received as much air at each inſpiration as is required for the purpoſes of reſpiration.

EXPERIMENT IV.

Three perſons of ordinary ſtature inſpired from the pneumatic veſſel thirty times ſucceſſively, and took in as much air at each time as the ſenſations in the breaſt ſeemed to require. The average quantity of air taken into the lungs at a ſingle inſpiration by the firſt, was — 12 cubic inches;
By the ſecond — 14 cubic inches;
By the third — 11.

Hence it appears, that the quantity of air required for a ſingle inſpiration is greater than could be expected from the preceding experiments: but this alſo varies a little in different perſons; and it is equally difficult to eſtabliſh a medium for inſpiration, as for expiration. We ſhall however take the medium quantity here at 12 cubic inches.

But the air which paſſes from the pneumatic veſſel into the lungs, goes from a cool to a warmer temperature; it muſt therefore undergo a degree of expanſion as ſoon as it gets into the lungs, and conſequently occupy a greater ſpace than before. This degree of expanſion may be meaſured, by incloſing a given quantity of atmoſpheric air in a glaſs receiver, diſpoſed in ſuch a manner as to indicate the alteration of temperature, and the proportional expanſion, at the ſame time.

EXPERIMENT V.

I provided a cylindrical glaſs receiver, with a thermometer ſuſpended in the middle; I meaſured the quantity of water the receiver contained, and divided the whole capacity into ſeveral hundred parts, which were diſtinguiſhed by correſponding marks on the outſide: then I inverted it in water, and introduced into it an hundred parts of atmoſpheric air at 69° of Fahrenheit, and applied heat to it gradually by means of warm water, until the thermometer in the inſide was raiſed to 98°: the volume of the whole was then increaſed $\frac{1}{6}$th. In ſeveral ſucceſſive repetitions the proportional expanſion was nearly the ſame.

If, then, we allow 12 cubic inches for a ſingle inſpiration, they will be increaſed to 14 cubic inches when they get into the lungs; therefore the volume of air before in the lungs receives an addition of 14 cubic inches by an ordinary inſpiration. But the volume of air in the lungs, before an inſpiration, was 109 cubic inches; hence it will be increaſed to 123 cubic inches; and the diſtention of the lungs muſt be changed in the ſame proportion: therefore the dilatation of the lungs after expiration is to their dilatation after inſpiration as 109 to 123 (*a*).

We next inquire into the effects of theſe different degrees of dilatation on the pulmonary veſſels, and on the current of the blood circulating through them.

Haller aſſerts, that the pulmonary veſſels are very much changed in the different ſtates of reſpiration; that they are conſiderably lengthened by inſpiration, and their angles and diameters ſo diſpoſed, as beſt to favour the circulation of the blood; but that they are greatly ſhortened by expiration, and their angles and diameters ſo much altered, as to produce a complete obſtruc-

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(*a*) That is, as 4,7769 is to 4,9732; the difference being only ,1963; which is not even two tenths of an inch.

obſtruction to the paſſage of the blood through them.

" In inſpiratione," ſays he, " pulmo qui " nunquam pleuram deſerit, per eosdem " paſſus quibus pectus dilatatur, et ipſe in " utroque diametro latior nunc fit, et in " ſpatium majus, ſed ſui ſimile augetur. " Id augmentum varie eſtimatum eſt; quin- " tuplo per inſpirationem, pulmonem am- " pliorem fieri clariſſ. Sauvages conjecit, " et inde decuplo.

" Vaſa ergo ſanguinea omnis generis cum " adtenſis bronchiis neceſſario extenduntur, " et flexiones alternæ in quas ea vaſa in ſe- " ipſa retracta, in ſtatu pulmonis minimo " ſe reciperant, eæ nunc in rectitudinem " exporriguntur :———Porro quæ ſibi in- " cumbebant proxima, ea a mutuo con- " tactu diſcedunt, et anguli inter diviſiones

"vaſorum majores fiunt, ſpatiaque adeo vi-
"cinis vaſis interponuntur.———Hinc in
"inſpiratione ſumma facilitas naſcitur ſan-
"guini de corde dextro exeunti.——In
"expiratione vero pulmo undique urgetur,
"et in multo minorem molem compri-
"mitur: vaſa ergo ſanguinea breviora
"quidem fiunt cum retractis bronchiis,
"eademq; anguſtiora nunc ſunt, ſiqui-
"dem pectus ſecundum tres ſuas dimen-
"ſiones arctatur.——Sanguis ergo quidem
"in pulmones undique comprimitur; et
"venoſus æqua vi preſſus, partim verſus
"arterioſum quidem reprimitur, eumque
"moratur aliquantum, partim verſus cor
"ſiniſtrum promovetur.——Quare in ex-
"piratione quam ponimus ſtabilem ſuper-
"eſſe, pulmonis pro ſanguine immeabilitas
"oritur, quam neque abſque palpitatione,
"et vitioſo conatu, demum omnino ullis
"ſuis viribus cor vincere queat."

Lib. VIII. Sect. 4.

But it appears, from theſe experiments, that the difference in the two ſtates of reſpiration is much leſs than Haller has repreſented; the changes produced on the pulmonary veſſels muſt conſequently be ſo likewiſe: the inferences he has drawn reſpecting the circulation of the pulmonary blood are therefore erroneous.

If we ſuppoſe the lungs dilated with the ordinary quantity of air in expiration (109 cubic inches), and then ſuppoſe 14 cubic inches more to be added, the dilatation of the lungs will be increaſed; but the increaſe will be uniform, and only in the proportion of 123 to 109; the pulmonary veſſels will be alſo extended uniformly in all directions (*a*), and their extenſion will be in the ſame

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(*a*) To ſuppoſe, with Haller, that the angles of theſe veſſels are changed, although the form of the lungs continues the ſame, is contrary to one of the fundamental principles of geometry.

ſame ratio: ſince then there is no other change produced on the pulmonary veſſels by reſpiration, but a difference in the degree of extenſion, and ſince this is ſo ſmall, the alteration in the ſum of their diameters muſt be equally ſo; and if the blood circulates through them in one ſtate, it muſt alſo in the other: and hence it follows, that the blood circulates through the pulmonary veſſels in all the degrees of natural reſpiration.

But, notwithſtanding this concluſion, it may be ſtill ſuſpected that the pulmonary blood does not circulate with proper freedom in all the different ſtates of reſpiration; that, in the ſtate of expiration, the current may be ſo much retarded, as to occaſion an accumulation in the veſſels of the right ſide of the heart, ſufficient to bring on an interruption or ſuſpenſion of ſome of the other functions. If it be ſo, the ſame effects will be produced by introducing a quantity of any fluid into the cavity of the thorax, to force the lungs to collapſe, and expel ſo much air as will reduce their volume much below the degree of ordinary expiration. This experiment is frequently made in the human body by diſeaſe: a quantity of watery fluid gets into the cavity of the thorax between the containing parts and the lungs, occupying a very conſiderable ſpace, and reducing their volume much below the dilatation of ordinary expiration; and ſtill there is no ſuſpenſion of any of the other functions. Many examples of this diſeaſe have been recorded by different writers, where the fluid was taken away ſeveral times from the cavity of the thorax, during the life of the patient: and there is one in the Memoirs of the Academy of Surgery, where the Author expreſſes much ſurpriſe that the blood could circulate through the lungs, while ſuch a quantity of fluid was confined in the thorax.

"Après les ſymptômes ordinaires," ſays the Author, "les chirurgiens pro~~non~~çoient, "que c'étoit un hydrothorax, & décident "ſur l'opération.

"Le malade étoit donc aſſis dans ſon lit, "le corps penché en avant, et ſoutenu par "pluſieurs aſſiſtans. Je lui fit la ponction "avec un trois-quart ordinaire. Le poinçon étant tiré, l'eau ſortit par la canule à "plein jet, et par des ſecouſſes qui répondoient aux mouvemens de la reſpiration. "Il en ſortit près de ſix pintes* d'une fluide. "Bientôt après, ſon pouls ſe ranimoit.— "Sept jours après, l'accumulation ſe faiſoit "encore;

(a) De France, *i. e.* 288 cubic inches.

"encore; & je tirai par la même opération "encore cinque pintes."

"Dans ces cas, le poumon, fort écarté "des parois de la poitrine, doit être pelotonné vers le centre, & réduit à un fort "petit volume, & ſes véſicules très-rétré-"cies.——C'eſt aſſez pour expliquer la "difficulté de la reſpiration.

"J'ai obſervé que toutes les fois qu'on "inſinuoit la ſonde de poitrine dans la ca-"pacité, on l'introduiſoit à la longueur de "quatre ou cinq pouces, ſans toucher ni "rencontrer aucune partie intérieure; & "c'eſt une choſe qui m'étonnoit toujours!"

Tom. II. p. 546.

I have frequently made a ſimilar experiment on ſeveral dogs, producing an artificial hydrothorax by introducing as much water through an oblique opening of the

intercoſtal muſcles into the thorax, as nearly filled one third of the whole cavity, cloſing the orifice afterwards. In all of them it brought on a conſiderable difficulty of breathing, but no other apparent inconvenience.

In theſe examples the volume of the lungs muſt have been conſiderably reduced, and the greateſt quantity of air they were capable of containing much leſs than the ordinary quantity in an healthy expiration; and ſtill the blood continued to circulate through the pulmonary veſſels in ſufficient quantity to keep up the action of the left ventricle of the heart, and the other functions of the body.

If, therefore, the blood circulates through the pulmonary veſſels with this freedom when the volume of the lungs is ſo much diminiſhed, it muſt certainly circulate through them with equal eaſe in expiration when the volume of the lungs is much greater: hence it follows, that the blood circulates through the lungs in expiration with ſufficient freedom to keep up the health of the ſyſtem.

From all theſe experiments we draw the following concluſions:

1°, The lungs contain 109 cubic inches of air after a complete expiration; and this quantity receives an addition of 14 cubic inches by inſpiration.

2°, The dilatation of the lungs after expiration is to their dilatation after inſpiration as 109 to 123.

3°, The blood circulates through the pulmonary veſſels in all the degrees of natural reſpiration.

4°, The circulation through them after expiration is ſufficiently free to keep up the health of the ſyſtem.

And hence it follows, *that the dilatation of the lungs is not the final cauſe of reſpiration.*

Another effect produced in the lungs by reſpiration, is a change in the chymical qualities of the air they contain. We proceed to inquire into the particular nature of this change, and its connexion with the other functions of the body.

SECTION IV.

To determine the Chymical Effects of the Air on the Lungs in Reſpiration.

IT has been long ſuſpected by philoſophers of the firſt character (*a*), that the air which we breathe undergoes ſome chymical changes in the lungs; and various conjectures have been propoſed concerning them, by different writers, at different times; but nothing more could be expected from the moſt penetrating mind, until chymiſtry put on the form of a ſcience: from that period the clouds of hypotheſis have been diſpelled, and the dawn of truth begins to appear. We ſhall not enter

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(*a*) Ariſtotle, Sir Iſaac Newton, &c.

enter into a detail of theſe different conjectures, nor of the progreſſive diſcovery of the facts connected with the ſubject; but rather endeavour to deſcribe the conſtituent parts of atmoſpheric air, as far as they are now known, and then inveſtigate the particular changes they undergo by reſpiration.

When atmoſpheric air is analyſed with the ordinary chymical teſts, it is found to be compoſed of phlogiſticated air, dephlogiſticated air, and fixed air. If any given quantity of atmoſpheric air (ſuppoſe 100 parts) be accurately analyſed in this manner, it is generally found to contain nearly two thirds of phlogiſticated air, one third of dephlogiſticated air, and a very ſmall quantity of fixed air: but the proportions of theſe airs are generally different in different parts of the atmoſphere, and ſometimes no fixed air can be found.

If 100 parts of analyſed atmoſpheric air be inſpired, and expired again into a receiver, it is found to have undergone a change in the proportion of its conſtituent parts: the quantity of dephlogiſticated air is diminiſhed, the quantity of fixed air increaſed, and the phlogiſticated air remains the ſame.

A celebrated chymiſt has propoſed to aſcertain the changes (*a*) which theſe airs undergo by a ſingle reſpiration (*b*): but the reſults of ſuch experiments are ſubject to ſome variations from the ſtate of the body, and from the duration of the reſpiration. Notwithſtanding theſe difficulties, I made ſeveral experiments on myſelf, to aſcertain the particular degree of theſe changes; and although there was always a little difference

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(*a*) Monſ. Lavoiſier.

(*b*) One inſpiration and one expiration.

in the reſults, yet by frequent repetition they became inconſiderable.

Firſt, I aſcertained the proportion of theſe airs in 12 cubic inches of atmoſpheric air; then I inſpired an equal volume of the ſame air, expiring it into a glaſs receiver, and analyſed the whole quantity. This was repeated ſeveral times, and the medium quantity was as follows:

The volume of air taken into the lungs at a ſingle inſpiration contained,	*Parts.*	The volume of air expelled from the lungs by the next ſucceeding expiration contained (*a*),	*Parts.*
Phlogiſticated air	80	Phlogiſticated air	80
Dephlogiſticated air	18	Dephlogiſticated air	5
Fixed air - - -	2	Fixed air - - -	13
	100		98

(*a*) In all theſe attempts I endeavoured to imitate the natural expirations; but the volume of air ex-

It appears, then, that the diminution of dephlogiſticated air, and the increaſe of fixed air, in a ſingle reſpiration, is conſiderable.

Let us next inquire if this diminution of the one, and increaſe of the other, be uniform and conſtant in the ſame volume of air ſeveral times reſpired: and, firſt, the diminution of the dephlogiſticated air.

This may be done by breathing a quantity of air ſeveral times from a glaſs receiver inverted in water, and by mixing a ſmall quantity of this air, after every expiration, with an equal quantity of nitrous air in the eudeometer of Fontana: the quantity of dephlo-

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pelled from the lungs was never equal to the volume of air taken in; ſometimes there was a diminution of $\frac{1}{50}$, and ſometimes $\frac{1}{60}$. This diminution was obſerved long ago by Boyle and Muſſenbroek.

dephlogiſticated air will then be indicated by the diminution of volume in the eudeometer.

I paſſed 12 cubic inches of atmoſpheric air into a glaſs receiver inverted in water, and put a meaſure of it into the eudeometer, and it occupied 100 parts; then I added an equal meaſure of nitrous air, and the whole volume of 200 parts was diminiſhed to — — — 144

Then I inſpired the whole of the volume from the receiver, and expired it in the uſual time; and on trying an equal portion of it in the eudeometer, the 200 parts were diminiſhed to — 158

After the ſecond expiration they were diminiſhed to — — 163

After the third, to — — 167

After the fourth, to — — 170

After the fifth, to — — 171

We might alſo determine whether the addition of fixed air be conſtant and uniform in ſeveral ſucceſſive reſpirations, by breathing a given volume of air in the ſame manner from an inverted receiver, and paſſing a ſmall quantity of it through lime-water after every expiration: but this would require great labour and minute attention; and ſince we know the quantity of fixed air added by a ſingle expiration, it is eaſy to determine whether there be any additions made to it afterward, by breathing the ſame air ſeveral times, and, after the laſt expiration, examining it with lime-water, and comparing the whole quantity of fixed air with the known quantity of a ſingle reſpiration.

I incloſed 12 cubic inches of air in an inverted receiver, and breathed it through a glaſs tube ſix times in ſucceſſion: on being examined with lime-water after the laſt expiration it contained 15 parts of fixed air. The ſame experiment was repeated, and the quantity of fixed air was 13 parts.

The diminution of dephlogiſticated air, and the increaſe of fixed air, is therefore conſtant and ſucceſſive in the ſame quantity of air frequently reſpired: but the changes in the ſucceſſive reſpirations bear no proportion to the changes in the firſt. Since, however, theſe changes are conſtant and uniform, they muſt be connected with ſome correſponding changes in the lungs equally conſtant and uniform; and there is no ſubſtance in the lungs where we can expect to find ſuch correſponding changes, except the blood which circulates through them.

Lower obſerved long ago in living animals, that the blood from a wound in the trunk of the pulmonary vein was florid;

and knowing before that the blood entering into the lungs by the pulmonary artery is black, he concluded that it acquired this florid colour by paſſing through the lungs; and finding afterwards that, when the animal ceaſed to breathe, the blood from the wound in the pulmonary vein was black, he attributed the florid colour to the action of the air in reſpiration (*a*). This opinion has been mentioned frequently ſince by different authors, and ſeems now to be generally believed.

To examine the fact with particular attention, I procured ſeveral large dogs, removed the ſternum, and expoſed the trunks of the pulmonary veins and arteries ſo as to allow me to diſtinguiſh accurately the colour of

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(*a*) Tractatus de Corde, p. 185.

of the blood paſſing through them. Then I inflated the lungs with a pair of bellows (after the deſcription of (*a*) Veſalius), imitating the natural reſpiration, and kept the animal alive by this proceſs for a conſiderable length of time. In theſe experiments it was obſerved, that, during the inflation, the blood in the trunks of the pulmonary artery was black, but in the trunks of the pulmonary veins it was florid: and when the inflation was intermitted for a minute, the blood in the trunks of the pulmonary vein became gradually black, like that in the arteries.

In ſome of theſe animals I divided the trunks of the humoral artery and vein; and during the inflation the blood from the artery

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(*a*) Veſalius de Corporis Humani Fabrica, Lib. VI. Cap. xix. p. 572.

artery was florid; but, on intermitting the inflation, it became gradually black, like the blood from the vein.

I alſo examined theſe appearances in the toad and the lizard, whoſe lungs conſiſt of only a tranſparent bladder, with blood veſſels ſo thin that the colour of the circulating blood may be eaſily diſtinguiſhed through them.

I inflated the lungs of theſe animals ſeveral times, and emptied them again by gentle preſſure, imitating by this method the reſpiration of the more perfect animals; and in all the examples, when the atmoſpheric air paſſed into the lungs, the blood in the pulmonary veſſels became gradually florid; but when they were emptied, it became gradually black: to this may be added the familiar circumſtance in blood-letting, the dark-coloured blood becoming florid after expoſure to the air.

All theſe facts ſeem to confirm the opinion of Lower, that the blood acquires a florid colour in paſſing through the lungs; and that this colour is produced by the chymical action of the air.

We next inquire by what part of the reſpired air this change of colour is produced, and what particular chymical proceſs takes place at the time.

Does it ariſe from the addition of fixed air ſeparated from the blood in paſſing through the lungs, or from the chymical action of the phlogiſticated or the dephlogiſticated air?

If the florid colour ariſes from the ſeparation of fixed air in its entire ſtate from the blood, then florid blood expoſed to a volume of fixed air in a cloſe veſſel muſt become black alſo.

I introduced four ounces of florid blood, freſh drawn, into a glaſs receiver containing fixed air, and I confined it there a conſiderable time; but it did not become black, nor undergo any ſenſible change. I then received ſome florid blood from the carotid artery of a ſheep into a phial filled with fixed air; but ſtill the florid colour was not altered: therefore the change of colour in reſpiration does not ariſe from the ſeparation of fixed air in its entire ſtate from the blood.

Nor can it be attributed to the chymical action of the phlogiſticated air; for it is generally known, that black blood, freſh drawn, expoſed to phlogiſticated air in a cloſe veſſel, undergoes no change of colour.

But it has been often aſſerted, that black blood freſh drawn, and expoſed to dephlogiſticated air, becomes florid.

To aſcertain the truth of this aſſertion, I incloſed a quantity of dephlogiſticated air in a glaſs receiver inverted in quickſilver, and introduced into it four ounces of blood freſh drawn from the jugular vein of a ſheep: the blood became inſtantly very florid, and the quickſilver ſeemed to aſcend a little in the receiver. To aſcertain this latter circumſtance, I repeated the experiment three or four times: the change of colour was always very ſudden, and after ſeveral minutes the quickſilver aſcended two or three lines. It is evident, then, that dephlogiſticated air changes the colour of black blood, and a ſmall portion of the air diſappears in the proceſs: but, as the changes in theſe experiments are ſimilar to thoſe in reſpiration, it might be inferred, that dephlogiſticated air produces the florid colour in both examples.

To ſatisfy myſelf of this, I inflated the lungs of ſeveral kittens with dephlogiſticated air, after the ſternum was removed; and the blood in all the pulmonary veins became immediately very florid.

Hence it is evident, that the florid colour acquired by the blood in reſpiration is produced by dephlogiſticated air. But it may be aſked, by what means the air is applied to the blood in reſpiration? whether by means of the abſorbents, or by the force of chymical attraction?

If it were taken in by the abſorbents, it muſt paſs directly to the right ſide of the heart, and change the colour of the blood there; which does not happen.

It has been ſhewn by Dr. Prieſtley, that atmoſpheric air will change the colour of blood through the coats of a bladder; but there are no direct proofs that it will produce the ſame effect through the coats of the veſſels in a living animal.

To determine this, I diſſected the cellular membrane from the ſmall veins in the neck of ſeveral rabbits, and confined the blood in them by ligatures; then I conducted a ſmall ſtream of dephlogiſticated air to the coats of the veſſels: in ſome of them the blood became a little florid; but in others there was no diſtinct change, although the ſtream of air was conducted for two minutes.

In theſe inſtances, however, where it took place, ſomething muſt have pervaded the coats of the veſſels; and this makes it highly probable, that, when the dephlogiſticated air produces the change of colour in the blood, ſomething pervades the coats of the pulmonary veſſels by the force of chymical attraction.

But what it is that pervades the veſſels, is not yet known: whether ſome principle ſeparated from the blood be combined with the dephlogiſticated air, to form fixed air; or whether the dephlogiſticated air be decompoſed, and a part of it paſſes into the blood, while the other part remains behind in the form of fixed air; or, laſtly, whether the dephlogiſticated air enters into the blood in its entire ſtate, whilſt the fixed air is ſeparated from the veſſels.

The greater number of facts in chymiſtry ſeem to favour the firſt ſuppoſition; but yet they only make it probable; and more than probability cannot be expected, until the nature of theſe airs be better underſtood.

But, whatever ſolution future experiments may furniſh, this general fact will remain unaltered; *viz.* That the change of colour which the blood undergoes in paſſing through the lungs, is occaſioned by the chymical action of the dephlogiſticated air of the atmoſphere; and that, in conſequence of this, fixed air is received.

To proceed in the inquiry, we endeavour to trace the connexion of this change in the pulmonary blood with the other functions of the body.

It is generally known, that when an animal breathes the ſame air ſeveral times, his pulſe becomes ſlower, till at length it ceaſes, and all the other functions are ſuſpended; and it is alſo known, that when reſpiration is obſtructed, ſymptoms very ſimilar are produced. How are theſe facts connected together?

It has been ſhown already, that, when the ſame air is breathed ſeveral times, or when it is retained in the lungs longer than uſual, there is a gradual diminution of dephlogiſticated air, and an increaſe of fixed air. Theſe ſymptoms then muſt ariſe from one of theſe circumſtances; either from the noxious quality of the additional fixed air, or from the want of the ſalutary quality of the abſent dephlogiſticated air: but the additional fixed air exerts no noxious quality in the lungs; becauſe it may be breathed in a much greater quantity without any inconvenience when mixed with atmoſpheric air: they muſt then be attributed to the gradual diminution of the dephlogiſticated air.

When the dephlogiſticated air is thus diminiſhed, the ordinary change of colour which the blood undergoes in the lungs will be alſo diminiſhed, till at length it will paſs through the pulmonary veins with the ſame black colour as when it entered into the arteries.

This has been already in a great meaſure demonſtrated in the examples of artificial inflation; and it may be ſeen ſtill better in the toad and the lizard, where the lungs may be expoſed a long time without deſtroying the life of the animal.

I forced a conſiderable quantity of air into the lungs of a ſmall dog, whoſe ſternum had been removed, and confined it there by a tight ligature round the trachea: the blood, continuing to circulate through the lungs in this ſtate, began immediately to put on a ſhade of brown in the trunks of the pulmonary veins; and in leſs than two minutes it became very black.

With the ſame view I provided a large toad, and expoſed his lungs on both ſides; and while they were filled with air, I immerſed him in a ſmall glaſs receiver containing water. When he was firſt put into the water, the blood circulating through his lungs was florid: when he had continued in the water twenty minutes, the lungs ſtill filled with air, the blood in all the pulmonary veſſels became gradually dark-coloured, till at length it was black. This was repeated ſeveral times with the ſame animal, and once or twice alſo with lizards; and in all the inſtances where they retained the air in their lungs a long time after immerſion, the pulmonary blood became gradually black.

It follows, therefore, that when the dephlogiſticated air is gradually diminiſhed, the blood paſſing through the veſſels of the lungs does not undergo the changes of colour which take place in ordinary reſpiration; and that the ſymptoms which follow obſtructed reſpiration muſt be attributed to this particular quality of the blood. But by what means it produces theſe ſymptoms, we next inquire.

The firſt ſuppoſition is, that it exerts ſome noxious quality on the nerves of the lungs, which is communicated by them to the brain.

If it operates in this manner, the ſame ſymptoms will not be produced by obſtructed reſpiration, when the nerves diſtributed on the lungs are ſo divided that their communication with the brain is intercepted.

To aſcertain this, I provided a ſmall dog, and divided the trunks of the par vagum and great intercoſtal nerves on each ſide of the neck about an inch below the larynx: the ſkin was then ſewed up, and the animal appeared to ſuffer no other inconvenience than a laborious reſpiration. The next day I put him under a glaſs bell inverted, and filled with atmoſpheric air. When he had continued there ſeveral minutes, the reſpiration became difficult; and ſoon after he fell down without any ſign of life. I divided the nerves of another dog in the ſame manner; and the day following I paſſed a ligature round the trachea, and obſtructed the reſpiration: ſoon afterward he fell down without any ſign of life.

It appears, then, that the black blood produces the ſame ſymptoms when the nerves of the lungs have no immediate communication with the brain; and hence, in the caſes of obſtructed reſpiration, it can exert no ſuch noxious quality on the brain by the mediation of the nerves.

From the lungs this black blood paſſes immediately into the ſinus venoſus and left auricle of the heart. What changes does it produce there?

The only ſenſible change it can produce in the heart, is in its contractions; and theſe may be eaſily obſerved by inflating the lungs, when the ſternum is removed, and the pericardium opened in ſuch a manner that the motions of the auricles and ventricles may be diſtinctly perceived.

I made theſe experiments with all the circumſtances here mentioned; and in the proceſs of inflation I attended carefully to the changes in the colour of the blood, and in the correſponding contractions in the left auricle and ventricle of the heart; and in all the examples, I obſerved that when the blood which paſſed into the left auricle was florid, the auricle and ventricle contracted ſtrongly, and the circulation went on as in health; but when the blood began to put on a ſhade of brown, the contractions were diminiſhed; and when it was black, the contractions ceaſed, although the auricle was diſtended with blood; and as the contractions ceaſed, the functions of the body were ſuſpended; but as ſoon as the florid colour began to be reſtored, the auricle and ventricle reſumed their contractions, and they gradually recovered their natural ſtate; and all the other functions returned.

In theſe examples the contractions of the left auricle and ventricle are immediately affected by the quality of the blood (*a*) paſsing into them; for when they ceaſe, the auricle is filled with black blood: but notwithſtanding theſe facts, to ſuch as have particular

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(*a*) This explanation has been objected to, by ſome perſons, on very reſpectable authority. It has been ſaid that the heart reſumes its contractions at the firſt inflation; and, as the blood in its cavity cannot be ſuppoſed to be changed ſo ſoon, the renewal of the natural contractions muſt ariſe from ſome other cauſe. But theſe two circumſtances are only aſſertions, which have not yet been demonſtrated. Beſides, ſome contractile motion may be produced in the heart by the agitation it ſuffers from the firſt diſtention of the lungs, in the ſame manner as feeble contractions are produced in the hearts of amphibious animals out of the body by agitation, or gentle compreſſion; but theſe motions are not ſo ſtrong as the natural contractions, and they ought to be carefully diſtinguiſhed from them.

particular opinions about the manner in which the blood acts upon the heart, it may not appear clear that theſe alterations in the contractions aroſe from the quality of the blood alone. This difficulty, however, may be removed by attending to ſimilar experiments in the amphibious animals (*a*), where

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(*a*) Some reſpectable writers have employed the word *ſympathy*, to expreſs or explain the connexion between the heart and lungs. When the lungs are inflated, the heart moves, they ſay, by ſympathy; but, as they have not defined the ſenſe in which the term is here employed, no one can judge of the propriety of their reaſoning. If by ſympathy they mean here a ſeparate principle or quality that is ſuppoſed to lie hid in the body, it is only a particular name for one of the occult qualities of the peripatetic ſchool, which cannot be proved to exiſt; and, as they convey no information to the mind, they have been long ſince baniſhed from all chaſte philoſophy.

where the heart has only one auricle and ventricle, where the pulmonary artery is a ſmall

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If the term be intended to expreſs a mechanical cauſe, to aſſert that the motion of the lungs in reſpiration keeps up the natural contractions of the heart mechanically, the aſſertion is not ſupported by facts: for, if it were ſo, the motion of the lungs would be alone ſufficient to keep up the contractions of the heart, and any kind of aërial fluid would be then equally effectual in reſpiration; and whenever the motion of the lungs ceaſed for a while, the contractions of the heart would always ceaſe alſo: but there are ſeveral kinds of aërial fluids which are not ſufficient for the purpoſes of reſpiration; for when animals breathe them ſeparately, the contractions of the heart ceaſe, even while the reſpiration continues; and when the amphibious animals are immerſed in water, the motion of the lungs ceaſes, yet the natural contractions of the heart continue upwards of an hour afterward; which could not happen, if the motion of the lungs occaſioned theſe contractions mechanically.

But

ſmall branch from the aorta, and the pulmonary vein proportionally ſmall empties itſelf into the ſinus venoſus and auricle along with the vena cava aſcendens, which carries the principal part of the blood. Here the quantity of blood brought by the vena cava would be ſufficient to keep up the action of the heart, independent of the pulmonary circulation, if quantity alone were

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But probably the term is uſed here in its original and more confined ſignification; *i. e.* to expreſs the co-exiſtence of effects, or the conſtancy with which one change in the body follows or accompanies another, without any regard to the efficient cauſe of either, or to the manner or probability of their connexion. In this ſenſe, as it expreſſes only the fact, no one can object to the uſe of it: but I cannot with propriety employ the word in this inquiry, as my object is to trace the manner of this connexion between the lungs and the heart, and to fill up the chaſm by inveſtigating their proper cauſes.

were required; and the coats of the ſinus venoſus and auricle, as well as thoſe of the blood veſſels, are almoſt tranſparent: the lungs alſo contain a quantity of air ſufficient to furniſh the neceſſary chymical changes to the pulmonary blood for a conſiderable time without any communication with the atmoſphere; ſo that the alterations which take place in the colour of the blood, and in the motions of the heart, in conſequence of obſtructed reſpiration, are more gradual and diſtinct than in the animals with double hearts, in which all the blood paſſes through the lungs.

I confined a large living toad on a plate of metal, with his belly upwards; then I removed a part of the ſternum, and his heart and lungs were expoſed to view: The lungs were then filled with air; the blood in the pulmonary veins was florid, and the heart contracted 44 times in a minute. In this ſtate he was immerſed in a ſmall quantity of tranſparent water, where the alterations in the colour of the blood, and in the contractions of the heart, could be accurately diſtinguiſhed. When he had remained in the water 15 minutes, the blood in the lungs began to put on a dark colour, and the contractions of the heart were diminiſhed to 30. In 15 minutes more the dark colour of the blood was increaſed, and the contractions of the heart were 18. The animal now made ſeveral ſtruggles to relieve itſelf, and threw ſome air out of its lungs; but the pulmonary blood becoming ſtill more dark coloured, the contractions of the heart were diminiſhed ſtill further, and in 40 minutes more they ceaſed; although the ſinus venoſus and auricle, and the trunk of the vena cava, were filled with black blood. The animal was now removed from the water, without any ſigns of life; but before the expiration of two minutes he opened his mouth, and took a large quantity of freſh air into his lungs. Soon after, he emptied them almoſt entirely; and this was repeated ſeveral times. During the proceſs, the blood in the pulmonary veins began to be florid, and the heart to renew its contractions; and in 15 minutes from the firſt inflation the contractions of the heart were 35, all the functions were recovered, and he walked about without any expreſſions of uneaſineſs.

This experiment was repeated ſeveral times on the ſame toad; and ſometimes, when he did not fill his lungs, after being removed from the water, we injected ſome air into them with a blow-pipe, and preſſed it out afterward, in imitation of the example he had given us; and by theſe means we renewed the contractions of the heart ſeveral times. But at length he was removed from the water, and ſuffered to remain an hour before freſh air was injected into his lungs. Soon after it was injected, the blood in the pulmonary veins became florid; but the heart did not again renew its contractions.

Theſe circumſtances were repeated afterward in the lizard; and in all the examples the contractions of the heart were diminiſhed in frequency, in proportion as the blood from the lungs became dark coloured; and they were renewed, when it became florid, in the ſame manner as in the experiment with the toad.

In all theſe examples the quantity of blood which came to the heart was fully ſufficient to keep up its contractions; and ſtill they were gradually diminiſhed in proportion as the pulmonary blood became darker; and when it was black, they ceaſed entirely. Theſe alterations, therefore, in the heart's motion, muſt ariſe from the quality of the blood only.

But it may be queſtioned, by what means the quality of the blood can diminiſh the motion of the heart? It muſt either exert ſome noxious power on the heart itſelf, or it muſt be an inſufficient ſtimulus to excite its contractions.

If it exerted a noxious power upon the heart itſelf, it muſt diminiſh or deſtroy the faculty of contraction (*a*); and if it diminiſhed or deſtroyed the faculty of contrac-

tion,

(*a*) By "faculty of contraction in the heart," I mean that quality by which it is diſpoſed to propel the blood through the circulating ſyſtem.

tion, the heart would not contract again when thoſe means are applied which are neceſſary to produce the contractions: but in all theſe experiments, where the heart ceaſed to contract when the blood which paſſed into it was black, as ſoon as ſome air had been received into the lungs to change a part of this blood to a florid colour, the contractions of the heart were renewed, and ſoon returned to their natural ſtate.

Therefore this black blood exerts no noxious power upon the heart itſelf; and we muſt conclude that, in all theſe caſes of obſtructed reſpiration, the heart ceaſed to contract, becauſe the blood which paſſed into it was an inſufficient ſtimulus (*a*).

And

(*a*) This concluſion may perhaps, at firſt, appear ſingular; as the ſame black blood is a ſufficient ſtimulus

And hence it follows, *That the chymical change which the blood undergoes in the lungs by*

ſtimulus to the right auricle and ventricle; becauſe it is expelled from them juſt before, when it enters into the lungs: and if it be a ſufficient ſtimulus to excite the contractions of the heart on the right ſide, why not on the left alſo?

But it muſt be remembered, that the two ſides of the heart do not reſemble each other exactly in all their qualities: there is a conſiderable difference between them, *both in reſpect to the quantity of muſcular fibre, and the facility of being excited to contraction*; and this alone is ſufficient to diſarm the objection.

But, even, if there were no ſuch difference, the inſtance is not peculiar to the heart; there are ſeveral examples in the animal body, where muſcles of a ſimilar ſtructure are not excited to contraction by the ſame or a ſimilar power: ſome of the muſcles are thrown into contraction by the will; ſome, by particular conceptions of the mind; and ſome, by chymical ſtimuli: yet none of theſe different powers will produce a perfect contraction of thoſe muſcles *by reſpiration, gives it a ſtimulating quality, by which it is fitted to excite the left auricle and ventricle to contraction.*

From all theſe experiments we draw the following concluſions:

1ſt, A quantity of dephlogiſticated air is ſeparated from the atmoſpheric air in the lungs by reſpiration, and a quantity of fixed air is added to it.

2dly, The dephlogiſticated air exerts a chymical action on the pulmonary blood; in conſequence of which it acquires a florid colour.

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to which they are not adapted by nature. Objections of this kind, therefore, do not apply immediately to our concluſions, but rather to the general law in animal bodies, which muſt be regarded at preſent as an *ultimate fact.*

3dly, In ordinary reſpiration this florid colour is ſeen diſtinctly as the blood paſſes into the left auricle; and the heart contracts then with its natural force and frequency.

4thly, When reſpiration is obſtructed, the florid colour is gradually diminiſhed, and the contractions of the left auricle and ventricle ſoon ceaſe.

5thly, This ceſſation of contraction ariſes from a defect of a ſtimulating quality in the blood itſelf.

And hence it follows,

That the chymical quality which the blood acquires in paſſing through the lungs, is neceſſary to keep up the action of the heart, and, conſequently, the health of the body.

SECTION V.

To determine the Nature of the Diſeaſe produced by Submerſion.

IT has been ſhown, in the Firſt Section, that animals confined in water throw out ſmall quantities of air, and then endeavour to take ſome in from the atmoſphere. By theſe efforts a portion of the ſurrounding fluid paſſes into their mouths, and often deſcends into the lungs; but the quantity is not ſufficient to produce the ſymptoms which afterward appear: (ſee the Experiments in the Second Section:) in the cavity of the lungs it mixes with the air, and increaſes the dilatation of the air-cells; in conſequence of which the lungs are in a middle ſtate of dilatation; that is, between expiration and inſpiration: and in this ſtate the blood circulates through the pulmonary veſſels with ſufficient freedom to keep up the health of the ſyſtem; (ſee the Concluſions in the Third Section;) therefore the ſymptoms are not produced by an obſtruction of the circulation through the lungs.

From the ſituation of the animal, the air and water are confined in the lungs; and the dephlogiſticated air which it contains is gradually conſumed; and conſequently the blood paſſing through the pulmonary veſſels receives leſs and leſs of its florid colour, and the contractions of the heart become proportionally ſlower, until they ceaſe entirely (ſee the Experiments and Concluſions of the Fourth Section), although the faculty of contraction ſtill remains.

It appears, then, that the ceſſation of the heart's motion may be fully accounted for from the action of the water preventing the entrance of the air into the lungs.

Let us next inquire, if the other particular ſymptoms (ſee the Firſt Section) can be traced to the obſtruction of reſpiration, and to the conſequent ceſſation of the heart, as their proper cauſe.

In proportion as the colour of the blood paſſing through the lungs is darker, the contractions of the left auricle and ventricle, and the *correſponding pulſations of the arteries, become weaker*, and the current of the blood ſlower; and whilſt the blood moves ſlowly in the larger trunks, it begins to ſtagnate in the ſmaller branches of the arteries and veins, where the reſiſtance to its paſſage is greateſt. At length, when the pulmonary blood is no longer fitted to excite the ſinus venoſus and auricle to contraction, *they receive it into their cavity, and remain at reſt.* As ſoon as they ceaſe to contract, and propel the blood to the head, *all the intellectual operations ceaſe* (*a*), *ſenſation and voluntary motion are ſuſpended, and the external ſigns of life diſappear*; and the black blood remaining at reſt in the arteries, and particularly in the ſmaller branches

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(*a*) Whatever may be the efficient cauſe of the intellectual operations, ſenſation, &c. the circulation of the blood through the brain is an indiſpenſable condition for them in all the more perfect animals. This is proved by the familiar examples of fainting from ſudden or great loſſes of blood; where, as ſoon as the motions of the heart can be perceived to ceaſe, conſciouſneſs, and all the other operations attributed to the mind, diſappear; and when the action of the heart is reſtored, they all return again.

branches of the arteries and veins, occaſions *the blue colour upon different parts of the body* (*a*), *but eſpecially about the face and lips,*

(*a*) It appears from the diſſection (ſee Sect. V.) that this particular blue colour of the face, &c. ariſes from the preſence of the black blood in the ſmaller branches of the arteries and veins; and it is ſtill further confirmed by the following remarkable caſe in the Obſervationes Anatomicæ of Sandifort, of the authenticity of which I have abundant confirmation from living teſtimony. The ſubject is a boy born in 1764.

" In the ſecond year of his age the nails of his fingers were obſerved to become blue; but this blueneſs often diſappeared, and returned again, for the ſpace of a year, when a general languor came on, and ſome livid ſpots were obſerved on his face, which increaſed very much by exerciſe, till ſoon after the whole face became blue on the ſmalleſt exertion of the body, and particularly the lips and tongue. Towards the latter end of the year he perceived *lips*, where the number of ſuperficial veſſels is moſt conſiderable. After the left auricle and

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perceived an anxiety about his breaſt, and complained of a general coldneſs of the body, which was not relieved by the application of external heat. Blood-letting relieved the anxiety in the breaſt: his blood was very black, and did not ſeparate properly into ſerum and craſſamentum. In the third year he perceived frequent palpitations of the heart, for which the cold bath and exerciſe were recommended; but they ſeemed to aggravate his complaints. Theſe ſymptoms continued ſtationary until the 10th year, when they were conſiderably increaſed, and he had a ſpitting of blood.

"In the 11th year he perceived an anxiety in his breaſt upon the ſlighteſt motion of the body, and he frequently fainted: the face alſo ſeemed to be a little ſwelled, and the blue colour was much increaſed. At length his legs became œdematous, and he died. On opening the thorax, it was found that the aorta took its origin from both the ventricles of the heart; one and a half of the valvulæ ſemilunares were and ventricle have ceaſed to contract, the right auricle and ventricle are ſtill expoſed to the action of thoſe means which naturally excite them to contraction, *and they continue to contract for ſeveral minutes*, and propel the black blood into the pulmonary arteries: this occaſions an accumulation in the pulmonary veſſels, and a livid colour on the lungs: but from the reſiſtance to the entrance of the blood into the pulmonary arteries, and from the want of the ſynchronic aſſiſtance of the left ventricle, the contractions of the right become gradually more feeble, until at length they ceaſe; and the right auricle, being ſoon after greatly diſtended

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were extended over the right ventricle, and one and a half over the left ventricle; ſo that half the quantity of blood which paſſed into the aorta was always black, not having paſſed through the lungs to undergo the alteration of colour."

diſtended with blood, ceaſes to contract alſo; yet the faculty of contraction ſtill remains.

Thus we have traced the connexion of the ſymptoms, and the conſequent ceſſation of the heart, to the obſtruction of reſpiration, as to their *proper cauſe*. It remains only now to aſcertain the name of the diſeaſe, and the place it ſhould have in noſology.

As the gradual diminution of the pulſe is a conſtant ſymptom of this diſeaſe, it was firſt called Syncope (*a*): but, from ſome peculiarities afterwards obſerved, phyſicians thought proper to diſtinguiſh it from the

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(*a*) Syncope. Motus cordis imminutus, vel aliquamdiu quieſcens. Noſol. Cullen. Genus 64.

the milder kinds of Syncope, and called it Aſphyxia (*a*); and this name has been generally retained. But another difficulty aroſe about the place it ſhould have in noſology; and, on account of the obſcurity of its nature, it was for ſome time excluded from every ſyſtem. At length it appeared probable, from the general maſs of obſervations and experiments, that the diminiſhed action of the heart and arteries aroſe from a morbid ſtate of the brain, occaſioned by the preſſure of ſome blood. In conſequence of this, the primary affection was called Apoplexy, and the diminiſhed action of the heart only a ſymptom; and the diſeaſe was placed under the genus

Apoplexia,

(*a*) Ασφυξια. Deletis omnibus vitæ indiciis, accedente etiam ſuffocatione, mortis imaginem ita refert ut meritò dubitetur, vitamne, an mortem predicare fas ſit.——Inſt. Pathol. H. D. Gaubii.

Apoplexia, where it ſtill remains (*a*). If, however, we are to conſider the primary morbid affection of the body as the diſeaſe itſelf (*b*), and all the conſequent appearances only as ſymptoms; this diſeaſe is in the blood, and conſiſts in the preſence *of this black blood in the left ſide of the heart and arterial ſyſtem*; and the diminution of the action of the heart and arteries, and the blue colour of the face, &c. &c. are only ſymptoms: this diſeaſe, therefore, might with more propriety be named Melanæma (*c*), and claſſed with others which have ſome reſemblance to it.

As

(*a*) Synopſis Noſolog. Cullen. pag. 196.

(*b*) Status ille corporis humani viventis, quo fit, ut actiones homini propriæ non poſſint appoſitè ad leges ſanitatis exerceri, Morbus dicitur.

Inſt. Pathol. H. D. Gaubii.

(*c*) Ex μὲλαν ἁιμα, ſanguis niger.

As there is no *Pyrexia* here, nor any *primary Neuroſis*, and as the alteration of colour in the ſkin is a conſtant ſymptom, would it not be more conſiſtent with the principles of noſological arrangement to place it in the claſs Cachexia, and order Impetigo? And ſince there is no genus for theſe diſeaſes, may we not propoſe one with with this name and definition?

MELANÆMA,

impedita ſanguinis venoſi in arterioſum converſio, cujus ſigna, Syncope, et livor cutis.

And this genus would afford an aſylum to ſeveral noſological wanderers that have not yet found a permanent reſting-place: for it is a natural conſequence of theſe experiments, that the diſeaſes which are brought on by hanging (*a*), by breathing fixed

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(*a*) It is ſomewhat extraordinary that the greater part of practitioners ſtill ſuppoſe the death, which follows fixed or phlogiſticated air, are all produced by this black blood going into the heart unchanged by reſpiration; and they ſhould be therefore placed in the ſame family (*a*): but, as name and arrangement are only of ſecondary importance, when the nature of a diſeaſe is aſcertained, it may perhaps be more

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follows hanging, to be produced by compreſſion on the brain; notwithſtanding the number of facts in ſurgery which ſhow, that a preſſure on the brain, much greater than could be ſuppoſed to ariſe from any quantity of blood accumulated there by hanging, will not diminiſh the contractions of the heart immediately, nor often for ſeveral hours, and even days, afterward.

(*a*) Perhaps in this manner:
Melanæma à Submerſione,
——— à Suſpenſione,
——— ab Inſpiratione Aëris fixi,
——— ab Inſpiratione Aëris phlogiſticati, &c.

more agreeable to general prejudice to retain the accuſtomed title Aſphyxia, and to arrange it as a ſymptomatic Syncope, with ſomething added to expreſs the primary affection: as

Aſphyxia à ſanguine venoſo in auriculam et ventriculum ſiniſtros tranſeunte.

Theſe, however, are only propoſals for the conſideration of Noſologiſts, which we willingly leave to their future determination.

SECTION VI.

To determine the Condition of the Body in this Diſeaſe, and the Means of diſtinguiſhing it from Death itſelf.

OF animal bodies there are only two general conditions, Life and Death; and ſince by *death* we underſtand the privation of life, there can be no intermediate ſtate between them. Of the body in this diſeaſe, we can ſay with propriety only, that it is alive, or that it is dead. If it were really dead, it would neceſſarily follow, that the means, which are employed to recover it, in the different experiments of the Fourth Section, muſt be ſuppoſed to communicate life to dead matter, which is impoſſible: the body is therefore alive, but with a degree of life leſs perfect than in the ordinary ſtate of health; and ſince a difference in degree does not occaſion an alteration in kind (*a*), the body muſt ſtill retain that principle which is the immediate cauſe of all the functions, that are performed in health (*b*); only it is not now excited to action; becauſe the external concomitant circumſtances, which operated upon it in health, are removed. Theſe external circumſtances are heat and reſpiration.

To

(*a*) Majus aut minus non variat ſpeciem.

(*b*) Hence it appears, that there is a ſtriking impropriety in the terms, which are commonly employed to expreſs the ſtate of the body in this diſeaſe, viz. "*ſuſpended life*," "*ſuſpended animation*," &c. and theſe terms ought to be laid aſide; becauſe they lead mankind to believe themſelves capable of *reanimating* or *reſuſcitating* a lifeleſs maſs, when they only cure a diſeaſe.

To aſcertain the ſeat of this principle, we inquire into the effects of the privation of heat and reſpiration on the living body.

It is generally known, that a ſmall diminution of the ordinary temperature of the body will not occaſion a ſuſpenſion of any of its functions: but it is alſo known, that a conſiderable diminution of the ordinary temperature of the body will occaſion a ſuſpenſion of the greater part of its functions (*a*). A certain degree of heat, therefore, in living bodies is indiſpenſably neceſſary to keep up the functions of health. But although the heat be indiſpenſably neceſſary, it is not in itſelf ſufficient to keep up theſe functions without reſpiration: for if heat be applied to the living body, when the

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(*a*) Flora Siberica, præfat. p. 72.

the different functions are ſuſpended, none of them will be recovered, until the reſpiration be eſtabliſhed; and the reſpiration is not always eſtabliſhed in this ſtate, in conſequence of the application of heat, but often requires artificial aſſiſtance (*a*). (See the Experiments with the amphibious animals in the Fourth Section.)

Since, then, the preſence of heat in the living body is not in itſelf ſufficient to keep up the different functions without reſpiration, the heat muſt not be regarded as the whole

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(*a*) No one can deny that the reſpiration is ſometimes eſtabliſhed in this ſtate, in conſequence of the application of heat. Reaumur mentions a caſe of this diſeaſe, where a perſon was recovered, only by being expoſed to the rays of the ſun: and Nature ſeems to employ the ſame means to recover the hybernating animals from their torpor, a ſtate very ſimilar to this diſeaſe.

whole cauſe of the continuance of theſe functions, but rather as putting the body in a condition to produce them, when the effects of reſpiration are exerted upon it. And hence, when the natural temperature is conſiderably diminiſhed in this diſeaſe, the body is deprived of a condition indiſpenſably neceſſary to favour the operation of reſpiration in reſtoring the functions of health.

From the want of reſpiration the heart ceaſes to contract; becauſe the blood, which paſſes through the lungs, is an inſufficient ſtimulus to it; and the heart ceaſing to contract, all the other functions are ſuſpended. But if the temperature and reſpiration be reſtored to the body ſoon after the ceſſation of the heart, the natural contractions will be renewed, and all the other functions recovered. And ſince the contractions are renewed by the application of the ordinary ſtimulus, the heart retains that principle (*a*), which is the immediate cauſe of its contractions, even after all the other functions are ſuſpended.

But if we attempt to reſtore the temperature and reſpiration to the body a conſiderable time after the ceſſation of the heart, its contractions will not be renewed, nor will any of the functions be recovered: and when the contractions are not renewed by the application of the ordinary ſtimulus, the heart has loſt that principle, which is the immediate cauſe of its contractions, and we have no means of reſtoring it.

From theſe facts it is evident, 1ſt, That the heart is the great ſeat of the principle of

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(*a*) The principle of life.

of life in all the more perfect animals; 2dly, That the contraction (*a*) of the heart with the ordinary ſtimulus is the only mark of the preſence of this principle; and when the heart contracts under theſe circumſtances, the body is alive; but when it does not contract under theſe circumſtances, the body is dead: Life in the more perfect animals may therefore be defined, *The faculty of propelling the fluids through the circulating ſyſtem.*

And whenever the functions of an animal are ſuddenly ſuſpended, and the body puts on the appearance of death, it is always in our power to determine whether it be really dead, by reſtoring the temperature, and

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(*a*) That action of the heart, by which it exerts the ordinary force to propel the blood along the circulating ſyſtem.

contractions of the heart; and the only means neceſſary to be employed are the application of heat to the body, and air to the lungs: but as the operation of theſe powers will be more or leſs effectual, in proportion as they are regulated by a regard to the circumſtances of the body, it will be neceſſary to deliver ſome particular directions for the application of them.

When we attend a perſon in this diſeaſe, the firſt buſineſs is to examine the temperature of the body, and, if it be conſiderably below 98°, to order the application of heat: but, ſince the ſcale of heat is ſo very extenſive, it is neceſſary to determine the degrees of it beſt calculated to promote the recovery.

It is generally known from ſeveral familiar facts, that whilſt the circulation of the blood continues, the temperature of the body may be raiſed many degrees above the natural ſtandard without deſtroying the principle of life: but it appears on the other hand, from the reſult of many attempts to recover the hybernating animals from their torpor, that when the circulation of the blood has ceaſed, and the temperature of the body is reduced near the freezing point, if heat be applied either very ſuddenly, or in a very high degree, the principle of life is ſoon deſtroyed; whereas, if it be applied gradually, and in a very low degree, to the ſame animals in the ſame circumſtances, the principle of life is often excited to action, and the functions are ſoon reſtored.

The effects of heat therefore are extremely different in different ſtates of health and diſeaſe; and the greateſt caution is required in deducing any inference from analogy to regulate the application of it as a remedy.

But ſince the condition of the body in this diſeaſe is nearly the ſame as in the ſtate of torpor, and ſince the progreſſion of the recovery is alſo the ſame; it can ſcarcely be unſafe to conclude, that heat will produce the ſame effects on the body in this diſeaſe, as in the torpid animals.

To favour the recovery then moſt effectually, the application of heat ſhould be conducted on the ſame plan, which Nature has pointed out for the torpid animals. It ſhould be applied very gradually and uniformly; and it may be raiſed to 98°, but not further than 100°.

When the body is warmed uniformly, and the heat of the interior parts about 98°, we direct our attention to the ſtate of the thorax; and if the patient make no attempt to inſpire, we proceed to inflate the lungs with air.

When a perſon is in health, the object of reſpiration is to change the quality of the blood paſſing through the pulmonary veſſels, and to fit it to excite the contraction of the left ventricle of the heart: but in this diſeaſe the pulmonary veins, the ſinus venoſus, and auricle, contain a quantity of blood, which has paſſed through the lungs without undergoing this neceſſary change: the firſt object therefore in inflating the lungs is to change the quality of the blood in the trunks of the pulmonary veins, ſinus venoſus, and auricle, to fit it for exciting their contractions. This muſt be done by introducing the air ſo deep into the lungs, that it may exert a chymical action on the blood in theſe cavities.

For this purpoſe, a great quantity of air muſt be introduced at each inflation; for if only 12 cubic inches be injected at a time, this ſmall quantity will occupy the larger branches of the trachea, and conſequently only a ſmall number of the pulmonary veſſels will be expoſed to its action: but if a much greater quantity of air be forced in at each time, ſome of it will paſs into all the more remote cells; and when they are thus uniformly diſtended, the pulmonary veins, the ſinus venoſus, and left auricle, will be expoſed as much as poſſible to its action, and ſome of the altered blood may be forced into them from the ſmaller veſſels.

On this account we ſhould introduce upwards of 100 cubic inches of air into the lungs of an adult at each inflation (*a*), and

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(*a*) This direction is further confirmed by the circumſtances of ſpontaneous recoveries from an ordinary Syncope: the firſt inſpiration is generally very full.

and it muſt be carefully drawn out again before more be introduced.

But one difficulty ſtill remains: there is ſometimes a quantity of water inſinuated into the ſmall branches of the trachea, and into the air-cells; (ſee the Second Section;) and if the lungs be inflated in this ſtate, the water will occupy thoſe places where the freſh air is principally required: and although the lungs be inflated with the utmoſt care and attention, it may be impoſſible, on this account, to apply the freſh air, ſufficiently near the ſinus venoſus and left auricle, to change the quality of the blood they contain (*a*): in all caſes, therefore, where the quantity of water is conſiderable,

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(*a*) I think I have ſeen this opinion verified ſometimes in young animals, where the lungs contained a conſiderable quantity of froth after ſubmerſion:

ſiderable, we muſt remove ſome of it, before the air is introduced. Sometimes a ſmall portion of it will paſs out by its own gravity, when the head is reclined; and ſtill more may perhaps be taken away by an inſtrument ſo contrived, on the principles of the pump, as to exhauſt the lungs of a part of their contents.

For this purpoſe, I propoſe the inſtrument A B C D E. The braſs cylinder A B contains

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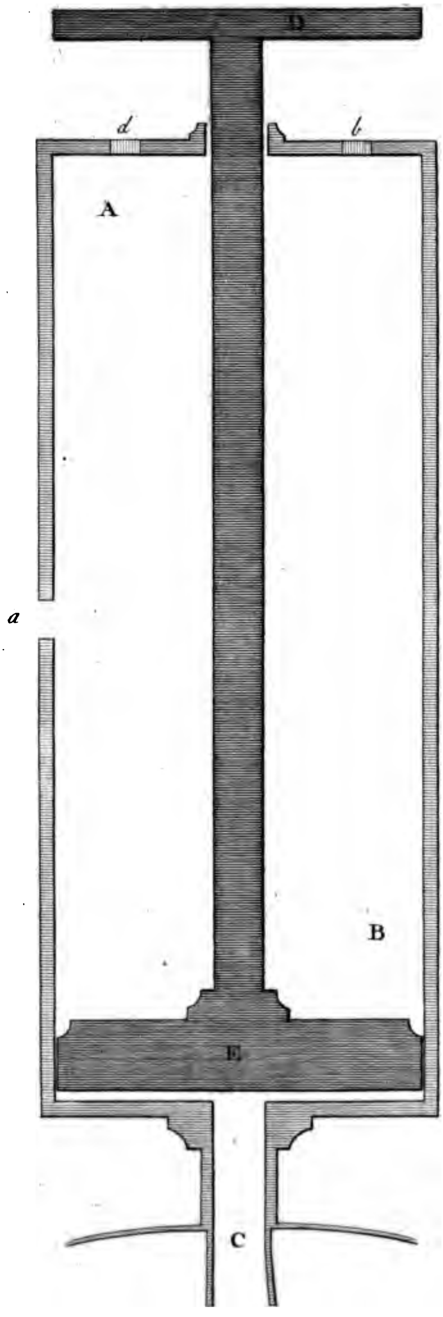
if I inflated them fully in this ſtate, the colour of the blood in the ſinus venoſus and left auricle was not apparently changed, and the heart did not renew its contractions, although it ſtill retained the faculty of contraction. Some ſimilar caſes are mentioned, by the Dutch writers, in the human ſubject after ſubmerſion. The perſons inſpired ſeveral times of themſelves, but ſtill they did not recover; and this failure may perhaps be accounted for in the ſame manner.


D
d
b
A
a
B
E
C

contains 100 cubic inches of air, and communicates with the atmoſphere by the ſmall circular opening *a*. The piſton D E is of wood, and lined with a ſoft ſubſtance at the bottom E, ſo as to be air-tight. The two openings *d*, *b*, are to allow the air to eſcape, when the piſton is drawn higher than the circular opening *a*. The tube C is for the attachment of a ſmaller one, to be inſerted into the noſe, larynx, or trachea (*a*).

If the lungs are to be inflated, the extremity of the ſmall tube muſt be inſerted into one of the aërial paſſages, and the others

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(*a*) I am favoured with this inſtrument by Dr. Nooth, a Gentleman diſtinguiſhed as much for liberality as genius, to whom the Arts are indebted for ſeveral valuable inventions, which are commonly attributed to others.

properly ſhut. The piſton being drawn up, and the orifice *a* cloſed with the finger, we preſs down the piſton, and force the contents of the cylinder into the lungs. A few ſeconds after, we draw up the piſton, and the air paſſes from the lungs into the cylinder again. Then we remove the finger from the orifice *a*, preſs down the piſton, and the greater part of this expired air eſcapes into the atmoſphere. After this, we draw up the piſton a ſecond time, while the orifice *a* continues open, and a volume of freſh air enters into the cylinder, which may be forced into the lungs in the ſame manner.

But where it is neceſſary to extract water from the lungs before the inflation, we begin the proceſs with the piſton down; and when the ſmall tube is inſerted, as before, we draw up the piſton, until the lower part

E is contiguous to the orifice *a:* the water will then riſe up from the lungs into the fauces, or cylinder. If the water riſe into the cylinder, it may be evacuated by detaching the tube C from its connexion with the ſmaller tube: and this may be repeated once or twice; but always with great caution, leſt the pulmonary veſſels be injured. Then we proceed to inflate the lungs in the manner juſt deſcribed (*a*).

If

(*a*) Other machines have been lately propoſed to inflate the lungs in this diſeaſe, and particularly one by Mr. Kite of Graveſend, and another by Mr. Hurlock of St. Paul's Church-yard; two ſurgeons of conſiderable eminence and reſpectability. I have not yet employed theſe machines on animals, but am informed, that they are extremely well calculated for the purpoſe. It appears, however, that they are encumbered with valves; and in the choice of an inſtrument for a buſineſs of this kind, which is ſometimes truſted to the awkward and ignorant

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If the inflations be continued carefully and ſlowly for ſeveral minutes in this manner, the contractions of the heart will be gradually renewed, and the other functions will ſoon return, without any other inconvenience than a difficult and ſterterous reſpiration, which often continues a ſhort time afterward: this inconvenience ariſes from ſome water ſtill remaining in the lungs, which will be gradually evaporated by the expired air.

In ſituations where dephlogiſticated air can be procured to inflate the lungs, it ſhould

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part of mankind, I ſhould be diſpoſed to prefer the moſt ſimple, where there are neither valves, nor ſtopcocks, and where the quantity of air to be injected is clearly meaſured: but the value of theſe different inſtruments muſt be determined by future experience.

ſhould be always preferred to atmoſpheric air. I have employed it in ſeveral inſtances in the ſmaller animals, and the recovery was commonly more expeditious than where atmoſpheric air was uſed; but I have not yet been able to recover an animal with dephlogiſticated air, where good atmoſpheric air had been ineffectual.

Several other remedies have been recommended for this diſeaſe by different writers, and many of them are ſaid to have been employed with ſucceſs; but as the circumſtances of their application, and the progreſſive changes in the body, are not minutely detailed, nothing can yet be ſaid to be determined in favour of their particular efficacy. If indeed we were to judge of them from the hiſtory of their introduction, or from their ordinary operation on living bodies, little aſſiſtance would be expected from them.

Some of them were ſuggeſted from a falſe notion of the nature of the diſeaſe (*a*); others, from a miſtaken opinion of the principal ſeat of life (*b*); and ſome, from an ignorance of the moſt effectual means to ſolicit the principle of life into action (*c*): and none of

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(*a*) Bleeding, friction, and ſuccuſſion. See Mémoires ſur les Noyés, &c. par Monſ. *Louis Nederlandiſche Jaarboeken*, April 1758. *Diſſertatio de Syncope*, *Auctore Hieron. Queye.*

(*b*) The application of different ſubſtances to the ſkin, the ſtomach, the inteſtines, the parts of generation, the noſe, the fauces, the extremities of the fingers, &c. *Diſſertatio de Cauſâ Mortis Submerſorum*, *Auct. Jacob Gummer.*

(*c*) The employment of electricity, alcohol, volatile alkali, tobacco, eſſential oils, and all the acrid and ſtimulating ſubſtances.

Tract. Iſnard. pag. 13. Obſ. 5.

Ranchinus in Tract. de Morb. ſubit. Cap. 12.

Ælius in Tetrab. Cap. 49.

Le Voyage d'Acadie, pag. 190.

of them appear fitted to excite the natural contractions of the heart by direct agency: but ſince they are now introduced into practice on the authority of ſeveral learned ſocieties, experiment alone ought to decide concerning their efficacy.

In the caſes recorded of their ſucceſsful application the bodies were warmed: now it has been ſhown in the Sixth Section, that the preſence of heat alone is often ſufficient to produce a recovery; and therefore, whilſt the remedies are applied under theſe circumſtances, the ſucceſs cannot with propriety be attributed to their operation: and if they be applied when the temperature of the body is ſo low, that warmth cannot be ſuppoſed to contribute to the recovery, it is known from experiment, that no ſenſible change will be produced: the concurrence of heat is therefore neceſſary to their ſucceſs.

Still, however, it may be ſuppoſed, that the concurrence of reſpiration is not neceſſary.

But this may be determined by applying theſe remedies in conjunction with heat, by attending to the progreſſive changes in the body during their ſucceſsful application, and by obſerving if the other functions return before the reſpiration be renewed.

I applied theſe remedies ſingly to ſeveral animals under theſe circumſtances, and attended carefully to the progreſſive changes in the body: ſome of them were recovered, and others were not; and in all the ſucceſsful caſes the progreſſion of the recovery was as follows. The organs of reſpiration began firſt to move, two or three inſpirations followed; the contractions of the heart were gradually renewed, and then the other functions followed.

In theſe caſes it appears, that theſe remedies were not often attended with ſucceſs; and even when they were ſo, the other functions did not return until reſpiration began: the concurrence of reſpiration is therefore neceſſary to their ſucceſs; and if it could be granted, that they entirely, or even in part, occaſioned the recovery in theſe caſes, it muſt evidently be by operating on the organs of reſpiration, by exciting them to action, by producing an inſpiration, and applying to the heart its natural ſtimulus; thus inducing it to contract, and to reſtore the ſuſpended functions.

Hence, if we allow to theſe remedies all the efficacy that partiality can claim, it can only be ſaid, that they ſometimes produce by indirect means, what may be always done directly by the method we have recommended: ſurely then we ought not to heſitate in preferring a remedy which can be applied directly, and which operates with certainty and expedition, to one that is ſlow, indirect, and uncertain.

In ſituations, indeed, where the means of artificial inflation cannot be immediately procured, theſe remedies may be employed for the moment; but every perſon ought to be appriſed of their inſufficiency, and of the neceſſity of employing artificial inflation, even after their application has failed.

Haller mentions a caſe, where the ordinary remedies had been employed an hour without any ſenſible effect, and yet by inflating the lungs the patient was quickly recovered (*a*); and I have often ſeen the ſmaller

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(*a*) Diſputat. ad Morb. curand. et Hiſt. pertinent. tom. vi. pag. 318.

ſmaller animals recovered by inflation, after the other remedies had failed.

Notwithſtanding theſe facts and concluſions, ſome practitioners may be ſtill diſpoſed to think with the ancients. With ſuch, authority will be more powerful than argument; yet, ſince thoſe who will not be ſhaken by the labour of youth, may ſometimes yield to the judgement of age, we ſhall venture to addreſs them in the language of the candid Sydenham.

" Atque hoc mihi, ſuffragante experientiâ
" multiplici, compertiſſimum eſt: experi-
" entiâ, inquam, optimâ duce, et magiſtrâ,
" ad cujus leges et normam niſi exerceatur
" Medicina, eam prorſus exulare ſatius eſſet:
" luditur enim (quod aiunt), de corio hu-
" mano plus ſatis, cum hinc empirici, ne-
" que morborum hiſtoriam, nec methodum
" medendi

" medendi callentes, et receptis tantum
" freti; iſthinc Sciolorum vaniſſimi ſpem
" omnem in affectato artis ambitu, et ſpe-
" culationibus utrinque pari fere momento
" diſceptatis ponentes. Ea demum Praxis,
" eaque ſola ægris mortalibus opem feret,
" quæ indicationes curativas ex ipſis mor-
" borum phænominis elicit, dein firmat
" experientiâ, quibus gradibus magnus Hip-
" pocrates ad cœlum aſcendit."

FINIS.

Fig:1

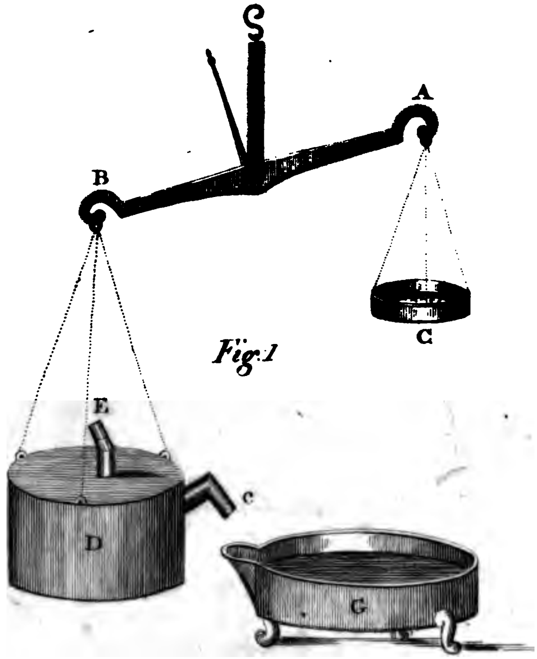
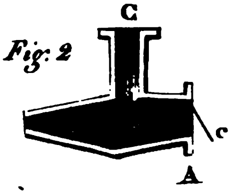


Fig: 2



AN

ESSAY

ON

RESPIRATION.

PARTS I. AND II.

BY JOHN BOSTOCK, M. D.

LIVERPOOL:
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1804.

TO

THE REV. JOHN YATES,

AS A TESTIMONY

OF RESPECT, GRATITUDE, AND

AFFECTION,

THIS ESSAY IS INSCRIBED,

BY

THE AUTHOR

INTRODUCTION.

ALL animals, however different in their ſtructure and functions, appear to agree in poſſeſſing organs appropriated to the purpoſe of reſpiration.*(a)* In man, quadrupeds, birds, and the amphibia, they conſiſt of a cavity, with the neceſſary appendages, which alternately receives and emits a portion of the air of the atmoſphere. Fiſh, in conſequence of the medium in which they are immerſed, perform this function by a different apparatus; they are furniſhed with a paſſage communicating with the fauces or œſophagus, and terminating in the external ſurface of the body, through which a part of the water received into the mouth is forcibly propelled. In this paſſage, the branchiæ, or gills, are ſituated, and the blood which circulates in their fringed extremities,

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(a) Note 1.

ligaments; it commences in the fauces, and deſcends into the thorax, where it divides into two branches, which communicate with the two diviſions of the lungs. Each of theſe is ſubdivided into ſmaller branches, called the bronchia, which again ramify into others ſtill more minute, until at length they terminate in what are called the veſicles, or air cells of the lungs. In man, and the warm-blooded quadrupeds, the cartilaginous rings are incomplete at the back part, where they lie contiguous to the œſophagus; this deficiency is filled up by cellular ſubſtance, furniſhed with muſcular fibres. Theſe fibres are placed both longitudinally and tranſverſely, ſo that by their contraction, the trachea is diminiſhed, as well in its length as in its diameter. The action of the longitudinal fibres is alſo aſſiſted by others, which paſs acroſs the intervals between each ſucceſſive ring. As the bronchia decreaſe in ſize, their cartilaginous rings become leſs and leſs apparent, and at length, when they terminate in the veſicles, their ſtructure is altogether membranous. A number of the air cells, with their accompanying veſſels, are connected together by cellular ſubſtance, and form what are called lobules; theſe are again united into larger lobules, and theſe into diviſions of more conſiderable ſize, which conſtitute the lobes of the lungs.(a)

The pulmonary ſyſtem of ſanguiferous veſſels, forms the moſt important part of the reſpiratory organs, and that to which all the reſt may be conſidered as ſubſervient. After the blood has completed its circulation through the body, it is returned by the vena cava to the right ſide of the heart. From this cavity, it is propelled through the lungs, along the branches of the pulmonary artery, which ramify with almoſt inconceivable minuteneſs over the bronchial air cells, forming what is called the rete mirabile of Malpighi; it is then collected by the pulmonary veins, and returned to the left auricle of the heart.

The pulmonary arteries and veins, with the air veſſels, and the accompanying lymphatics and nerves, are all connected together by cellular ſubſtance, and compoſe the lungs, two ſpongy maſſes which completely fill the thorax; the internal ſurface of this cavity is lined with a membrane, called the pleura, which is reflected over the ſurface of the lungs; it is aſcertained, that in all the different ſtates of reſpiration, the two parts of the pleura

(a) Note 4.

pleura are in perfect contact.(a) The diaphragm is a ſtrong, expanded, and peculiarly irritable muſcle, which ſeparates the contents of the two great cavities of the body; in its natural ſtate, it aſſumes an arched form, convex with reſpect to the thorax, but when in a ſtate of contraction, its curvature is diminiſhed, and conſequently the cavity of the thorax becomes increaſed.

(a) Note 5.

CHAP. II.

A Deſcription of the Mechaniſm of Reſpiration.

RESPIRATION conſiſts in the reception and emiſſion of air into, and out of the bronchial veſſels of the lungs, by means of the alternate enlargement and contraction of the cavity of the thorax. When the body is in its natural poſition, and the muſcles are relaxed, the lungs are in a ſtate of expiration; but after they have continued in this condition for only a few ſeconds, we are under the neceſſity of inhaling a quantity of freſh atmoſpheric air. For this purpoſe the diaphragm is contracted, and thus rendered leſs convex towards the thorax, by which means the capacity of the cheſt is augmented. The intercoſtal muſcles are, at the ſame time, thrown into action; and by elevating the arch of the ribs, and thus increaſing the diſtance between the ſternum and the ſpine, ſtill farther tend to enlarge the dimenſions of the thorax. The diaphragm is, however, the chief agent in producing this effect;(a) and anatomiſts are generally agreed in ſuppoſing, that the uſe of the intercoſtals conſiſts principally in fixing the lower ribs, ſo as to prevent their being drawn down by the action of the diaphragm, and thus obviating the effects of its contraction.(b) The contraction of this muſcle in ſome degree diſplaces the viſcera which lie contiguous to it, and puſhing them againſt the parietes of the abdomen, cauſes a protruſion of the ſtraight and oblique muſcles which compoſe its anterior boundary. The capacity of the thorax being augmented, and a free paſſage being left by the trachea for the admiſſion of the air, a portion of it will neceſſarily ruſh into the lungs, in order to maintain an equilibrium between the external and internal preſſure.(c) The contraction of the diaphragm and intercoſtals ſoon ceaſes, the elaſticity of the cartilages of the ribs re-acts, and the parts return to their natural ſtate; at the ſame time a degree of contraction takes place in the muſcles of the abdomen, and the viſcera are reſtored to their former poſition; the thorax is thus reduced to its original capacity, and a portion of air is conſequently expelled from the lungs.(d)

(a) Note 6. (b) Note 7. (c) Note 8.

(d) Note 9.

We have now been deſcribing what takes place in an act of ordinary inſpiration; but this function, though ſo eſſential to life that it cannot be interrupted for the ſhorteſt interval, without imminent danger, is, at the ſame time, ſo far under the control of the will, as that, according to inclination or neceſſity, it may be exerciſed in very different degrees. When we are deſirous of making a full inſpiration, beſides the diaphragm and intercoſtals, we call into action the external muſcles of the breaſt, thoſe of the ſhoulder and other neighbouring parts, which, by elevating the ribs and ſternum, ſtill farther increaſe the capacity of the thorax.*(a)* When, on the contrary, we wiſh to produce a full expiration, the abdominal muſcles are more ſtrongly contracted, the viſcera are thus puſhed more forcibly againſt the diaphragm, and its convexity towards the thorax is increaſed.

The grand object of reſpiration is to effect a change in the blood, by the operation of the atmoſpheric air; in order to accompliſh this end, it is neceſſary, both that a freſh ſupply of air be continually offered to the blood, and alſo that as large a ſurface of blood as poſſible be preſented to the air.

(a) Note 10.

air. The fabric and mechaniſm of the lungs are adapted to produce theſe effects in the moſt complete degree: the very act of reſpiration conſiſts in perpetually changing the air of the lungs, and the manner in which the blood of the pulmonary artery is diſtributed over the ſurface of the veſicles, in branches of almoſt inconceivable minuteneſs, affords the beſt opportunity to each particle of blood for undergoing the action of the air. Several phyſiologiſts, and among others Dr. Hales and Dr. Keill,(a) have attempted to eſtimate the number of the bronchial veſicles, and the extent of the membrane lining them. The reſults differ ſo much as to demonſtrate the fallacy of their calculations; but they agree in conceiving the membrane to be many times more extenſive than the ſurface of the whole body.

(a) *Keill, Tent. Med. Phys. p.* 80. *Hales's Stat. Essays. v. i. p.* 241.

CHAP. III.

Inquiry into the Bulk of a single Inspiration, and into the Capacity of the Thorax in its different States of Distension.

AFTER having obſerved the manner in which the act of reſpiration is effected, it naturally becomes an object of attention to determine the capacity of the lungs, and the quantity of air received and emitted by them. There are ſeveral circumſtances which render the latter part of this inquiry of peculiarly difficult reſearch, and notwithſtanding the labors of ſeveral ſkilful experimenters, the object of their inveſtigation has not been ſatisfactorily aſcertained. Owing to a difference in ſtature, and in the peculiar conformation of the thorax, probably alſo in the conſtitution and habits of individuals, great varieties prevail in the quantities of air reſpired by different perſons; and in the ſame perſon it is well known, that the reſpiration is materially influenced by the degree of muſcular exertion, the ſtate of the ſtomach, the mental impreſſions, and the powers of volition; all, therefore, that can be effected by experiment, is to aſcertain the average quantity of air reſpired in that ſtate of the body, when it is leaſt under the influence of external agents.

Many calculations were made by the older writers, to determine the capacity of the lungs, and the change which it experiences by reſpiration, but as their eſtimates were founded rather upon hypothetical reaſoning than upon experiment, they were for the moſt part remote from the truth. Borelli is the earlieſt phyſiologiſt who eſtabliſhed an experimental inquiry into the quantity of air received by a ſingle inſpiration.*(a)* Dr. Jurin afterwards improved upon this method, and obtained reſults which appear to have been tolerably accurate.*(b)* His experiments conſiſted ſimply in receiving the air which was expelled from the lungs into a bladder, and by repeating this proceſs for a ſufficient number of times, he concluded, that the quantity of air employed in a ſingle act of reſpiration amounts to 40 cubic inches. No additional information ſeems to have been

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(a) De motu Anim. p. 2. *prop.* 81.

(b) Halleri El. Phys. lib. viii. *sect.* 3. 8. *Hales's Essays v.* i. *p.* 243.

been acquired from the time of Jurin until the publication of Dr. Goodwyn's Eſſay on the Connexion of Life with Reſpiration.*(a)* This author enters fully into the diſcuſſion of the queſtions reſpecting the capacity of the thorax, and the quantity of air received in a ſingle inſpiration, and he inſtituted a number of ingenious and elaborate experiments for the purpoſe of aſcertaining theſe points.

His work commences with an attempt to meaſure the quantity of air left in the lungs after a complete expiration. Every animal, he remarks, immediately before death, produces a full expiration, and therefore, by aſcertaining the capacity of the thorax in the dead ſubject, we obtain a ſolution of the propoſed inquiry. The diaphragm being the only part of the cheſt which remains moveable, he endeavoured to fix it by applying a firm compreſs about the upper part of the abdomen. An opening was then made into the thorax, and the lungs, collapſing by their natural elaſticity, expelled the air which they previouſly contained, and thus left a cavity between the pleuræ, which he filled with water. This water he conceived would exactly meaſure the ſpace previouſly occupied

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(a) Note 11.

occupied by the air contained in the lungs, and from the average of four different experiments, he concluded the quantity of air left in the lungs after a complete expiration, to be 109 cubic inches.*(a)*

Dr. Goodwyn next attempted to aſcertain the quantity of air received into the lungs by a ſingle inſpiration, and for this purpoſe he uſed the following apparatus.*(b)* D is a veſſel with two openings, through one of which E he inſpired, while the other c was immerſed in water contained in the diſh G; a quantity of water equal to the volume of air inſpired will neceſſarily paſs into the veſſel D, and from the weight of this water, it is eaſy to eſtimate the bulk of the air taken into the lungs. By taking the average of thirty inſpirations, and by uſing the neceſſary precautions, the quantity appeared to be twelve cubic inches, which when it entered the lungs, he ſuppoſed would, by their higher temperature, be expanded to fourteen cubic inches: fourteen cubic inches therefore he eſtimates to be the quantity of air employed in an ordinary act of reſpiration. Having previouſly found the quantity

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(a) Note 12. *Connexion of Life with Respiration, p.* 26.

(b) Vide Fig. 1.

tity of air left in the lungs after expiration to be 109 inches, he concludes, that the difference of capacity which the lungs experience in the alternations of reſpiration, is in the proportion of 109 to (109+14) 123.*(a)* Theſe numbers are much leſs than the quantities aſſigned by preceding writers, particularly by Dr. Jurin; it will therefore be proper to examine how far the experiments and obſervations of Dr. Goodwyn may be found objectionable.

The poſition, that every animal makes a complete expiration before death, which forms the baſis of his experiments, is in the main true; yet there is every reaſon to ſuppoſe, that it will require many reſtrictions before it can be admitted as the firſt ſtep in a phyſiological calculation. According to the nature of the diſeaſe or accident producing death, the ſtate of the lungs muſt vary conſiderably. In many inſtances, the reſpiration is directly impeded by an obſtacle preventing the ingreſs and egreſs of the atmoſpherical air out of the bronchia; in ſome caſes, the air when admitted, is prevented from undergoing its appropriate change from the action of the blood, while in a large claſs of diſeaſes, life is extinguiſhed by the

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(a) P. 28 *&* *seq.*

the gradual ceſſation of the powers, which enable the heart to propel the blood through the pulmonary veſſels. To determine preciſely, what is the different condition of the lungs in theſe different circumſtances, would require a long and difficult inveſtigation; it is ſufficient for the preſent purpoſe to ſtate theſe differences, as it is evident that they muſt produce different effects upon the organs of reſpiration.

The method which Dr. Goodwyn employed to retain the form of the thorax, lies open to ſome objections. With whatever care the compreſs be applied about the region of the diaphragm, there is ſtill reaſon to believe, that a ſufficient power might puſh it down upon the viſcera contiguous to it, and ſo cauſe them to protrude into the loins, the groin, or ſome other part of the parietes of the abdomen, to which the bandages were not applied. Mr. Coleman farther urges againſt Dr. Goodwyn's experiments, that after a complete expiration, the diaphragm is raiſed as high as the 4th or 5th rib, but that a compreſs about the upper part of the abdomen, would be incapable of retaining it in this poſition, and that the ribs themſelves will alſo deſcend by the preſſure of the water admitted into the cavity of the thorax, and will conſequently draw down the whole ſubſtance of the diaphragm. *(a)*

Dr. Goodwyn appears alſo to have been miſtaken in conceiving, that a complete collapſe of the lungs is produced by the admiſſion of water into the cavity of the thorax. *(b)* A degree of preſſure will in this caſe be exerciſed upon the ſurface of the lungs, which will produce a material contraction in their bulk, but it will not obliterate the cavities of the bronchial veſicles, an effect which muſt take place before all the contained air can be expelled. Both from the ſhape and texture of theſe organs, and from actual experiment, we know that ſcarcely any degree of external force can ſo far evacuate the contents of the lungs, as to render them ſpecifically heavier than water. *(c)* Upon the whole we may conclude that Dr. Goodwyn's eſtimate of the capacity of the lungs after a complete expiration is not unobjectionable, yet the experiments muſt be conſidered as valuable, and as affording reſults probably not very remote from the truth.

The author will be found to be leſs fortunate in

(a) Coleman on Resp. p. 89. *(b) Goodwyn, p.* 24.
(c) Boer. prælect. t. v. *p.* 2, *p.* 458. *Petit, Mem. Acad.* 1733.

in his ſecond ſubject of inquiry, viz. reſpecting the quantity of air received by a ſingle inſpiration. He remarks himſelf, that in breathing through the tube E, the water is raiſed out of the veſſel G, contrary to the power of gravity, and that a greater effort is therefore neceſſary to receive the due quantity of air into the lungs. *(a)* He conſequently attempted to obviate this ſource of inaccuracy, but the nature of the apparatus ſeems to render this impracticable. Dr. Menzies has alſo pointed out another cauſe of error in this experiment; when the mouth is removed from the tube, the external air will immediately ruſh in and repel part of the water into the veſſel G, and conſequently the meaſure of an inſpiration will be eſtimated below the truth; that this actually takes place Dr. Menzies found by adding valves to the apparatus. *(b)*

A more important error into which Dr. Goodwyn has fallen, conſiſts in the comparative eſtimate which he forms of the capacity of the lungs in the ordinary ſtates of inſpiration and expiration. He calculates the quantity of air left in them after a complete expiration to be 109 cubic inches, and he ſuppoſes that 14 inches are received

(a) Goodwyn, p. 33, 4. *(b) Menzies on Reſpiration, p. 19.*

received at each inſpiration ; hence he infers, that in the ordinary proceſs of reſpiration, the lungs experience a dilatation and contraction from 123 to 109 cubic inches. *(a)* But it is obvious that the author here confounds together an ordinary and a complete expiration.

The quantity of air emitted from the lungs is, within certain limits, under the control of the will, ſo that we are enabled by a voluntary effort to expel a conſiderable quantity of air after an ordinary expiration. The amount of this quantity has been variouſly eſtimated, from 220 to 70 cubic inches; we ſhall probably be not far from the truth in fixing it at 170 cubic inches. *(b)* Yet it is certain, that after the moſt powerful voluntary effort of expiration, the lungs will not be completely emptied, they will then be only reduced to the ſtate in which Dr. Goodwyn made his experiments, when he found them to contain 109 cubic inches.

Admitting therefore the juſtice of his calculations, the inference which he draws from them will be inadmiſſible; the difference of capacity in the ſtates of expiration and of inſpiration will

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(a) Goodwyn, p. 36, 7. *(b)* Note 13.

be, the quantity of air that can be expelled after an ordinary expiration, (which I have eſtimated at 170 cubic inches) added to 109, the quantity ſtill remaining in the lungs, in all 279 cubic inches; and 279 added to 14, the meaſure of a ſingle inſpiration, equal to 293 inches; but we ſhall find that this meaſure of a ſingle inſpiration is not correct. From this view of Dr. Goodwyn's work it appears, that though his experiments were ingeniouſly contrived, and appear to have been carefully executed, yet that ſome of his moſt important concluſions are erroneous.

A ſhort time after the appearance of Dr. Goodwyn's work, Dr. Menzies publiſhed his Eſſay on Reſpiration. He begins by inſtituting an inquiry into the quantity of air employed in a ſingle act of reſpiration, and alſo into the capacity of the thorax, as Dr. Goodwyn had previouſly done; but he arrives at concluſions which are materially different, and which there is every reaſon to conſider as much more nearly approaching the truth. He procured an allantoid capable of containing 2400 cubic inches, which was fixed to a tube furniſhed with two valves. *(a)* The allantoid was placed at the valve c, ſo as to receive

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(a) Vide Fig. 2.

ceive only the air of expiration, and he continued breathing into it until it was completely filled. By comparing the number of expirations with the bulk of the allantoid, he calculated that each conſiſted of ſomewhat more than 40 cubic inches. (a) This reſult he confirmed by fixing another allantoid filled with air to the tube C, and from obſerving the number of inſpirations which this ſecond allantoid afforded, he found the bulk of each inſpiration to correſpond nearly with his previous eſtimate of a ſingle expiration. Not ſatisfied with theſe calculations, Dr. Menzies had recourſe to experiments of a different kind. A man was placed in a large veſſel of water, warmed nearly to the temperature of the blood, and the degree of aſcent or deſcent of the water in the upper part of the veſſel being accurately noted, from theſe data it was eaſy to form an eſtimate of the alteration in ſize which the thorax undergoes during reſpiration. (b) The trials made with this apparatus correſponded remarkably with thoſe performed by means of the allantoid, and thus afforded a ſtriking proof of their correctneſs. In the experiments of Dr. Menzies there appears indeed no aſſignable cauſe of error, and their coincidence with each other, and alſo with thoſe of

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(a) *Menzies, p.* 30. (b) *p.* 24—30.

Jurin mentioned above, muſt be conſidered as a powerful argument in favor of their accuracy.

In attempting to eſtimate the capacity of the lungs, Dr. Menzies adopts Dr. Goodwyn's eſtimate of 109 cubic inches, as the meaſure of the air left in them after a complete expiration, and adding to this quantity 70 cubic inches, which he found he could ſtill expel after an ordinary expiration, he ſuppoſes 179 inches to be the bulk of the thorax in the ſtate of ordinary expiration; to this he adds 40 cubic inches, which gives 219 as the capacity of the lungs after an ordinary inſpiration.(a) Theſe calculations of the bulk of the thorax are probably below the truth, as there is no doubt that conſiderably more than 70 inches can be expelled from the lungs by employing a powerful effort of expiration.

Mr. Davy in his reſearches concerning the nitrous oxide, performed ſome experiments for the purpoſe of aſcertaining the capacity of the lungs in their different ſtates of diſtenſion, and the quantity of air received into them by a ſingle inſpiration. In order to determine this latter point, he merely practiſed reſpiration in a peculiar pneumatic apparatus, until he had acquired the power of breathing in it without any extraordinary labour or attention, in a perfectly natural manner. From a number of trials he conceived, that at each inſpiration he took into the lungs no more than 13 cubic inches. *(a)* He alſo attempted to diſcover the capacity of his thorax in a ſtate of complete expiration; he proceeded to the ſolution of this problem in the following manner. He endeavoured to prove by previous experiment, that when a quantity of hydrogene is reſpired it is neither abſorbed by the blood nor in any way altered, but is ſimply mixed with the air contained in the lungs. He accordingly inſpired a quantity of this gas, and then making a complete expiration, he aſcertained what quantity of the hydrogene he had diſcharged from his lungs, and what proportion it bore to the other gaſes expelled at the ſame time. From theſe data he determined what quantity of hydrogene was ſtill retained, and ſuppoſing it to have been uniformly mixed with the whole air in the lungs, he eaſily calculated the abſolute volume of air ſtill remaining in them. Making the due allowances for the expanſion which it would experience from the heat of the body, he ſuppoſes that the capacity of his lungs in a ſtate of complete expiration amounts to no more than 41 cubic inches. *(a)*

(a) *Menzies p.* 32.

(a) *Reſearches, p.* 433.

It is difficult to reconcile theſe eſtimates with thoſe of Dr. Menzies, which were apparently deduced from accurate and well contrived experiments. With reſpect to Mr. Davy's method of determining the bulk of a ſingle inſpiration, we may remark in general, that the quantity of air taken into the lungs varies ſo much at different times, and depends in ſo great a degree upon the will, that other circumſtances being the ſame, thoſe experiments will ſtand the beſt chance of accuracy in which an average is taken from a conſiderable number of inſpirations; in this particular the experiments of Dr. Menzies poſſeſs a decided ſuperiority. There is alſo another circumſtance to be noticed with reſpect to the peculiar apparatus employed by Mr. Davy, which may be ſuggeſted as a probable cauſe of inaccuracy. He informs us that he attempted to breathe in the mercurial air-holder in ſuch a manner that the reſpiration might require no unuſual effort, and he concludes, that in this caſe the function would be exerciſed in the ſame manner as in its natural ſtate. But in breathing into this apparatus,

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(a) *Reſearches, p.* 409.

ratus, notwithſtanding the nice adjuſtment of its parts, ſome degree of reſiſtance is to be overcome, which does not exiſt in ordinary reſpiration, and therefore if the ſame degree only of exertion be employed, a leſs quantity of air will certainly be emitted from the lungs. It would require a number of ſucceſſive reſpirations greater than appears to have been employed by Mr. Davy, before any painful ſenſation will ariſe from this impediment; uneaſineſs in the organs of reſpiration is produced more readily by an alteration in the quality than the quantity of air received into them.

The eſtimate which Mr. Davy has formed of the capacity of the lungs, in the ſtate of complete expiration, differs ſo materially from other direct experiments, and is ſo incompatible with the ideas which are derived from a conſideration of the anatomical ſtructure of the thorax, that it is impoſſible to avoid ſuſpecting the exiſtence of ſome error or fallacy in the proceſs which he employed. The only conjecture which occurs to me, in any degree likely to remove the difficulty, is the ſuppoſition, that the hydrogene was not diffuſed uniformly through all the cavities of the lungs; a ſuppoſition which is in ſome degree countenanced by the conſideration of the different ſpecific gravities of the gaſes, and which derives confirmation from the experiments of M. Jurine, of Geneva. He received the air of one expiration into four different veſſels, and he found the different portions of air to exhibit different chemical properties. According to the order in which they had been expelled from the lungs, they contained a greater proportion of oxygene, and a ſmaller of carbonic acid. Hence it may be conjectured, that in the experiment of Mr. Davy, the hydrogene which he had inſpired, had not mixed itſelf with the air contained in the veſicles, in the ſame proportion as in the larger branches of the trachea, conſequently the air upon which the experiment was made, would contain a greater proportion of hydrogene than the average of the whole air contained in the lungs, and the eſtimate of their capacity, taken from the quantity of hydrogene, would be neceſſarily below the truth. *(a)*

The experiments which I have related of Goodwyn, Menzies, and Davy, are the moſt accurate that have been performed upon the mechaniſm of reſpiration; it will however be proper to notice the eſtimates of ſome other authors, even though we may find them to be leſs ſatisfactory.

(a) Note 14.

An intereſting account is given in the Encyclopedie Methodique *(a)* of ſome experiments performed by M. Jurine, on the chemical change produced upon the air by reſpiration in different conditions of the body. In the courſe of the detail, he is led to mention the bulk of a ſingle inſpiration, which he eſtimates at 20 cubic inches; this the editor, M. Hallé, conſiders as too large a quantity; but there is no mention made of the reaſons upon which the opinions either of M. Jurine, or of M. Hallé are founded. Mr. Kite, in his eſſay on ſubmerſion, ſtates the quantity of air received by an ordinary inſpiration to be 17 cubic inches, *(b)* and M. Delametherie, the learned editor of the Journal de Phyſique, reduces it to a much leſs quantity, *(c)* in neither inſtance, however, are we informed of the experiments by which theſe reſults were obtained. Mr. Abernethy employed an inverted jar filled with mercury, and by reſpiring as much as poſſible in the ordinary manner, he expelled from the lungs 12 cubic inches. *(d)*.

Mr. Coleman, in the courſe of his work on ſuſpended reſpiration, is led to compare the capacity of the lungs after drowning, with their capacity in a ſtate of health, and thus to diſcover the diminution which the cavity of the thorax experiences in a ſtate of complete expiration. He conducted his experiments by applying a ligature about the trachea of an animal which he had previouſly drowned, he then detached the lungs from the body, and preſſed out all the air into an inverted jar of water. He afterwards inflated the lungs, and noted the quantity of air they were capable of receiving when fully diſtended; theſe two quantities he compared together *(a)*. The reſults of his experiments were very different from what might previouſly have been expected; the lungs of a cat, which when inflated were capable of holding 16 drachms of air, after drowning, were found to contain only half a drachm; and in a dog, which he killed by hanging, the proportions were ſtill more extraordinary, nearly as forty-three to one. We have no reaſon to doubt either the faithfulneſs with which theſe experiments are related, or the accuracy with which they were performed, yet, as the lungs always exactly fill the cavity of the thorax, they cannot be reduced in capacity without a proportionable reduction in the ſize of the cavity containing

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(a) *Article Medecine, t.* i. *p.* 493, *&c seq.* (b) *p.* 47.

(c) *Journal de Physique, t.* xlvi. *p.* 108.

(d) *Essays, p.* 142.

(a) p. 91. *&* *seq.*

containing them, and it ſeems almoſt inconceivable, that ſo enormous a difference could take place in the capacity of the thorax of the ſame animal. (a) We may therefore conjecture that there exiſts a ſource of fallacy ariſing from the nature of Mr. Coleman's experiments. I ſhall point out ſome circumſtances which may, in part at leaſt, remove the difficulty.

When an animal is killed by interrupting the paſſage of the air into the bronchial veſſels, whatever may be conſidered as the immediate cauſe of death, it is found, that a conſiderable portion of the air which is left in the lungs is converted into carbonic acid gas. According to the plan upon which Mr. Coleman conducted his experiments, a part, at leaſt, of this gas muſt neceſſarily have been abſorbed by the water through which it was received, and upon which it was lodged; conſequently his eſtimate of the quantity of air left in the lungs will be leſs than the truth. This abſorption of carbonic acid gas may tend in ſome degree to reconcile the apparent contradiction in Mr. Coleman's experiments, but the circumſtance which I ſhall next point out is more extenſive in its operation, and will have a much more important effect upon our reaſonings.

(a) Note 15.

When an animal is in the act of drowning, the ſenſe of ſuffocation which is experienced produces a violent effort to expire. All the muſcles which can conduce to this effect are brought into ſtrong action, and contracted to the utmoſt, in order to expel from the lungs as large a quantity as poſſible of the air which is contained in them. Theſe muſcles, however, becoming exhauſted by their efforts, are quickly relaxed, and the thorax returns nearly to its natural dimenſions. In conſequence of the ſituation in which the animal is placed, the external air has no acceſs to the lungs, and on this account the air which remains in them becomes conſiderably rarefied. Owing to this circumſtance, a portion of the mucous fluid which lines the membrane of the veſicles is converted into an aqueous gas, which is mixed with the rarefied air, and fills the cavities of the lungs. *(a)* The uneaſineſs of the animal continues to encreaſe, and after a ſhort time, a ſecond violent effort to expire enſues, and a part of this rarefied air, together with ſome of the aqueous vapour, is forced out. This proceſs is repeated until a large proportion of the air originally contained in the lungs is excluded from them, and the powers of the animal being completely exhauſted,

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(a) Note 16.

hauſted, the reſpiratory organs ceaſe to act. It is obvious on the firſt view of the ſubject, that in the method of experimenting employed by Mr. Coleman, all the aqueous vapour was entirely diſregarded in his eſtimate of the contents of the lungs. When they were removed from the thorax and expoſed to the preſſure of the atmoſphere, the greateſt part of this vapour would be immediately condenſed, and any portion of it which ſtill retained its elaſtic form, would be totally deſtroyed in its paſſage through the water contained in the inverted jar *(a)*. It may be farther remarked, that owing to the circular ſhape of the bronchia and veſicles, and more eſpecially to the cartilaginous texture of the former, it is impoſſible by mere preſſure to expel all the air which they contain. When they have once been rendered pervious by inſpiration, no degree of compreſſion can render them ſpecifically heavier than water; it is even doubtful whether this effect can be produced by placing them in the vacuum of the air pump: *(b)* but it does not appear that theſe precautions were employed in the experiments now under conſideration.

From the circumſtances which I have pointed out,

(a) Note 17.

(b) Petit Mem. Acad. 1733, *Boer Prælect. t.* v. *p.* 458.

out, it is evident that Mr. Coleman has eſtimated below the truth the capacity of the lungs in their ſtate of complete expiration, and there is reaſon to apprehend, that in determining their capacity in the ordinary condition of inſpiration, he has fallen into the oppoſite error. In order to aſcertain this point, he detached the lungs from the body, and after diſtending them with air, meaſured their contents by preſſing it, as in the former experiment, into a graduated veſſel filled with water. From the ſtructure of the lungs, and the elaſticity of their ſubſtance, it is not improbable that when they were in this manner fully diſtended, they would be ſtretched to a ſize beyond what they occupied while contained in the cavity of the thorax. (a)

Theſe remarks upon the method by which the proceſs of reſpiration is effected, will ſhew, that though phyſiologiſts are agreed as to the mechaniſm by which the air is received into and ſent out of the lungs, yet that with reſpect to their relative capacity in the different ſtates of reſpiration, their opinions and experiments are much at variance.

Before we can arrive at a perfect knowledge of this part of the ſubject, it will be neceſſary to aſcertain preciſely the 5 following points. 1ſt. The quantity of air received into and ſent out of the lungs in an ordinary act of reſpiration. 2d. The proportion which this bears to the quantity of air contained in the thorax after an ordinary expiration, which may be conſidered as the natural condition of the thorax. 3d. The quantity of air which can be ſtill expelled after an ordinary expiration; and 4th. The proportion which this bears to the air ſtill left in the lungs, which will meaſure their ſtate of complete expiration. Laſtly. The quantity of air which can be taken into the lungs by the greateſt effort of inſpiration.

(a) *Boer. Instit.* § [illegible] *Blumenbach Instit. Phys. p.* 11.

The firſt of theſe points is, I apprehend, aſcertained by the experiments of Dr. Menzies. Though the calculations of other phyſiologiſts are conſiderably different, yet, there ſeems no obvious ſource of inaccuracy in the proceſſes which he employed, and the coincidence of the different experiments with each other is certainly too remarkable to be merely the effect of accident. Reſpiration is an act ſo much influenced by external cauſes, that we can never expect to attain any certainty as to the quantity of air ordinarily received into the lungs, without taking the average of a conſiderable number of inſpirations. By means of the allantoid, Dr. Menzies was enabled to compare together 56; in this reſpect alone his experiments poſſeſs a decided ſuperiority over thoſe of any other phyſiologiſt. The coincidence which took place between the reſults of his experiments performed with the allantoid, and thoſe in which a man was placed in a veſſel of warm water, convey to the mind an almoſt irreſiſtible conviction of their truth. We may therefore conclude, with reſpect to the firſt of theſe points, that 40 cubic inches is the quantity of air employed in an ordinary act of reſpiration. *(a)*

The proportion which an ordinary expiration bears to the quantity of air ſtill left in the lungs has been variouſly eſtimated. The calculations of the older writers upon this ſubject ſeem to have been very incorrect. It appears that in performing their experiments they removed the lungs from the thorax, and then obſerved what quantity of air they were able to force into them, in order to their complete diſtention; but the uncertainty of this mode of proceeding is ſufficiently obvious. Dr. Menzies eſtimated the medium capacity

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(a) Note 18.

capacity of the lungs in a ſtate of ordinary expiration at 179 cubic inches; I have already ſtated the reaſons which induce me to conſider this quantity as under-rated, and to conceive that the lungs in their natural condition contain about 280 cubic inches of air.

The experiments and obſervations which have been made to determine the quantity of air left in the lungs after a complete expiration, have been pretty fully detailed, and it appears, that notwithſtanding the ingenuity and labour which have been exerciſed upon this point, it muſt ſtill be conſidered as undetermined. Upon the whole, Dr. Goodwyn's eſtimate of 109 cubic inches is the neareſt approximation to the truth, though, probably, not altogether correct.

From the above data it may be eſtimated, that by each ordinary expiration $\frac{1}{7}$ part of the whole contents of the lungs is diſcharged, and that by the moſt violent expiration, ſomewhat more than $\frac{4}{7}$ of the air contained in them is evacuated. Suppoſing that each reſpiration occupies about 3 ſeconds, (a) a bulk of air nearly equal to three times the whole contents of the lungs will be

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(a) *Haller*, viii. 4, 29. *Thomson's Chemistry*, *v.* iv. *p.* 485.

expelled in a minute, or about 4114 times their bulk in 24 hours. The quantity of air reſpired during the diurnal period, will be 1152000 cubic inches, or 666⅔ cubic feet.

CHAP. IV.

Inquiry into the cause of the first Inspiration, and of the Alternation of Inspiration and Expiration.

IN viviparous animals reſpiration commences immediately after birth, when the circulation which had been previouſly carried on between the mother and the fœtus is interrupted. (a) The manner in which the commencement of this important action is effected, and the change which is then produced in the animal conſtitution, ſo as to render this function abſolutely neceſſary for the continuance of exiſtence, have been frequently made the ſubject of inveſtigation. The latter of theſe inquiries is the foundation of the celebrated problem which was propoſed by Harvey for the conſideration of his contemporaries. "Quomodo nempe embryo," he aſks, "poſt "ſeptimum menſem in utero matris perſeveret?

(a) Note 19.

"cum tamen eo tempore excluſus ſtatim reſpiret; "imo vero ſine reſpiratione ne horulam quidem "ſupereſſe poſſit; in utero autem manens ultra "nonum menſem, abſque reſpirationis admini"culo, vivus at ſanus degat." (a) This queſtion may be ſtated ſtill more generally; why is the animal which has once reſpired, under the neceſſity of continuing the reſpiration without intermiſſion; when, if the air had never been received into the lungs, the ſame animal might have remained for ſome time without exerciſing this function?

Several of the older phyſiologiſts propoſed anſwers to this queſtion, but they were founded either upon theories which are now univerſally diſcarded, or upon miſtaken notions reſpecting the uſe of reſpiration. Dr. Whytt's hypotheſis is the firſt which I ſhall notice; it is not indeed free from objections, but it deſerves attention on account of the reputation which it long maintained in ſome of the moſt diſtinguiſhed ſchools of medicine.

This author commences his inquiry into the cauſe of the firſt inſpiration by remarking, that before birth, the blood of the mother is properly prepared for the uſe of the fœtus, but that when the communication between them is interrupted, it becomes neceſſary for the young animal to perform the requiſite alteration in its fluids, by means of its own reſpiration. As the uneaſy ſenſations of hunger and thirſt excite in us a deſire of food, when it is wanted for the nouriſhment of the body, ſo immediately after birth, from the want of freſh air in the lungs, there ariſes an uneaſy ſenſation about the thorax, which may be conſidered as the appetite for breathing. This appetite he conſiders as natural to the newly born animal, and the expanſion of the cavity of the thorax is effected by the ſentient, active principle, in order to prevent the fatal conſequence which would enſue from the deficiency of air in the lungs. Dr. Whytt proceeds to obſerve, that this appetite for air commences only at birth, becauſe, in conſequence of the ſtruggles of the fœtus at this period, its circulation will be conſiderably quickened, and the lungs will receive a larger ſhare of blood than previouſly paſſed through them ; this ſtimulates them into action, and ſeems to be the immediate cauſe of the appetite for breathing. He farther urges, that the circumſtance of the animal being now immerſed in a medium which is proper for reſpiration, and having the air applied to the face, mouth, and noſtrils, may cauſe it to attempt this new action of the thorax. Upon the whole he conſiders breathing "as owing to a peculiar ſenſation of "the body, which determines the mind or ſenti"ent principle to put certain muſcles or organs "in motion."

(a) *Harveii Exerc. de gen. anim. p.* 361. *Haller*, viii. 5, 1.

With reſpect to the problem propoſed by Harvey, which was mentioned above, he obſerves it "to be of ſo very eaſy ſolution, that it is not "a little ſurprizing, that many phyſiological "writers ſhould have attempted it in vain." When the fœtus has once breathed, he remarks, that the blood which formerly paſſed through the foramen ovale & ductus arterioſus, now paſſes through the veſſels of the inflated lungs, and will ever after continue to flow through them. If, therefore, the reſpiration be diſcontinued, the blood will ſtagnate in theſe veſſels, and the animal will be ſuffocated, becauſe, according to his hypotheſis, the alternate motion of the lungs in reſpiration, is abſolutely neceſſary to carry on the circulation of the blood through them. *(a)*

Haller conceives that the fœtus, having before birth been in the habit of ſwallowing a portion of

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(a) On Vital Motions, Sect. 9.

of the fluid in which it is immerſed, after it leaves the uterus, continues to open its mouth for the reception of nutriment, when a portion of the air with which it is now ſurrounded, is neceſſarily taken into the lungs. The veſicles being thus expanded, permit the blood to paſs through the pulmonary veſſels, which was formerly tranſmitted by the foramen ovale and ductus arterioſus. The lungs then acquire the condition of thoſe of other breathing animals, and like them require a regular ſupply of freſh air to prevent the blood from ſtagnating in its paſſage from the right to the left ſide of the heart. *(a)*

The following ſolution of this difficulty is propoſed by the celebrated author of the Zoonomia. Dr. Darwin imagines that the power of deglutition is acquired by the fœtus before it leaves the uterus, in conſequence of its repeated attempts to ſwallow a part of the liquor amnii, in which it is immerſed ; but the act of deglutition is of ſo different a nature from that of inſpiration, that he thinks this latter cannot be acquired before birth. When the circulation is interrupted between the mother and the fœtus, that diſagreeable ſenſation is experienced by the infant, which ariſes

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(a) Note 20.

ariſes from the want of freſh air; all the muſcles of the body are put in motion to relieve this anxiety; and among others, thoſe of the breaſt, ribs, and diaphragm. The action of theſe being found to anſwer the deſired end, "reſpiration is "diſcovered," and the ſame action is ever after repeated when the diſagreeable ſenſation recurs. *(a)* It will be unneceſſary to enter into a formal refutation of theſe theories; though they may be entitled to the praiſe of ingenuity, they are evidently built upon the aſſumption of principles which are at leaſt doubtful, if nor untenable. The queſtion concerning the commencement of reſpiration is ſo intimately connected with the ideas which we entertain reſpecting its uſes, that we can ſcarcely expect a complete ſolution of the difficulty until we have previouſly aſcertained the purpoſes which this function ſerves in the animal œconomy. There are, however, ſome peculiarities in the ſtructure and mechaniſm of the reſpiratory organs of the fœtus, which it will be proper to notice in this place.

The lungs of an animal which has never reſpired, are of a firm and compact texture, and ſo condenſed, as to have a ſpecific gravity greater than that of water; whereas, no mechanical preſſure is found ſufficient to evacuate them, ſo far as to cauſe them to ſink in water, after they have been once inflated. *(a)* In this ſtate the pulmonary arteries and veins receive only ⅓ part of the quantity of blood which circulates through them, after they have been diſtended by the act of inſpiration. *(b)* The cavities of the bronchia and air veſicles are nearly obliterated, and that part of them which remains pervious, is filled with a peculiar mucous fluid. *(c)* As the lungs are in all caſes contiguous to the parities of the thorax, it is obvious, that the ſize of the cavity containing them, while in this condenſed ſtate, muſt be proportionably contracted. Accordingly we find that the arch of the ribs is depreſſed, by which means the ſternum is made to approach nearer to the ſpine, the diaphragm is puſhed up into the higher part of the cheſt, and its concavity towards the abdomen is conſiderably greater than it is ever found to be in an animal which has once reſpired. *(d)*

(a) *Zoonomia, v.* i. *Sect.* 16, 4.

The ſpace occupied by the lungs themſelves is ſtill farther diminiſhed by the great ſize of the heart in the fœtus, and by the interpoſition of the thymus

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(a) Boer. prælect. t. v. *Pars* 2*a. p.* 457. *Petit, Mem. Acad.* 1733. *(b) Haller in Boer. Præl. t.* ii. *p.* 172. *(c) Petit, ubi supra. (d) Senac, Mem. Acad.* 1724.

thymus between the laminæ of the mediaſtinum. *(a)* This contracted ſtate of the thorax is in part alſo produced by the poſture of the fœtus before birth ; the head is bent forwards, ſo that the chin reſts upon the breaſt, the ſpine is arched, and the knees are raiſed up to meet the face. *(b)* The abdominal viſcera are therefore puſhed ſtrongly againſt the diaphragm, and ſo far increaſe its curvature, that a conſiderable portion of them is forced into the region of the thorax : the liver, which in the fœtus is unuſually bulky, *(c)* is entirely concealed by the ribs. As ſoon however as the animal leaves the uterus, its poſition is changed, the limbs and the trunk are ſtraightened, and the preſſure conſequently removed from the thorax and abdomen ; the elaſticity of the cartilages of the ribs being at liberty to act, raiſes the arch of the thorax, and thus encreaſes the diſtance between the ſternum and the ſpine ; the liver and the reſt of the abdominal viſcera fall down into their proper ſituation, and permit the diaphragm to deſcend and aſſume the curvature which is natural to it after birth.

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(a) Haller, xxix. 4. 33. *do. in Boer. Præl. t.* v. *Pars 2a, p.* 459.

(b) Harveii Exer. de gener. anim. p. 353. *Denman's Midwifery, v.* i. *p.* 293.

(c) Haller, ubi supra.

While theſe changes are going forward in the relative poſition of the contents of the thorax and abdomen, the increaſed ſize of the former cavity cauſes the air to ruſh through the open mouth of the animal into the bronchial veſicles; whence the blood, meeting with leſs reſiſtance to its paſſage through the veſſels of the lungs, than through the foramen ovale is propelled along the pulmonary artery. The reſpiratory organs are then in their natural condition, or in the ſtate of moderate expiration, and the neceſſity for an inſpiration depends upon the ſame cauſe, which renders the alternation of expiration and inſpiration eſſential to the future exiſtence of the animal.

It appears therefore that the firſt degree of expanſion which is produced in the lungs of the fœtus, is not effected by muſcular contraction, but depends merely upon the removal of external preſſure, which permits the different parts of the thorax and abdomen to aſſume their natural poſition; the conſequence of this new ſituation of the viſcera is an encreaſe of the capacity of the cheſt, and this ſpace is immediately filled with a portion of the ſurrounding air. Any farther encreaſe of the ſize of the thorax muſt be effected by the contraction of the diaphragm and intercoſtals; perhaps in ſtrict propriety this muſcular contraction ought to be conſidered as the firſt beginning of the action of reſpiration.

Nearly allied to this inveſtigation concerning the commencement of reſpiration, is a queſtion which has alſo been the ſubject of much diſcuſſion among phyſiologiſts, viz. what is the immediate cauſe of the ſucceſſive alternations of inſpiration and expiration?

The ſolutions which have been attempted of this queſtion, have been no leſs numerous than thoſe reſpecting the cauſe of the firſt inſpiration, but as the ſubject is in itſelf perhaps more obſcure, ſo we ſhall find the explanations which have been advanced, ſtill leſs ſatisfactory. I ſhall mention the opinions of Haller and Whytt, which are the leaſt exceptionable, and are ſtill received by many reſpectable phyſiologiſts.

Haller, proceeding upon the ſuppoſition that in expiration the paſſage of the blood through the lungs is impeded, obſerves, that a preſſure upon the brain, and a reflux of the blood through the veins, will be produced from this circumſtance. Hence ariſes a neceſſity for encreaſing the capacity of the thorax, in order to remove the compreſſion of the pulmonary veſſels, and in conſequence of a painful ſenſe of anxiety or ſuffocation, proceeding from a partial ſtagnation of the blood, the will calls into action the muſcles of inſpiration, which remove the impediment to the tranſmiſſion of blood through the lungs, and conſequently the painful feeling which was cauſed by the obſtruction. The ſame uneaſy ſenſations are however produced by inſpiration, when protracted beyond a certain period; the muſcles therefore, which were acting to enlarge the thorax, ceaſe to contract, their relaxation is followed by a ſtate of expiration, which continues until the reſiſtance to the paſſage of the blood recurs, and brings back the conſequences ſtated above. Haller conſiders the action of the muſcles of reſpiration as altogether voluntary. *(a)*

Dr. Whytt offered an explanation in many reſpects ſimilar to that propoſed by Haller. He conceives, that the paſſage of the blood through the pulmonary veſſels, is impeded by expiration, and that a ſenſe of anxiety is produced from this cauſe; this unpleaſant feeling acts as a ſtimulus upon the nerves of the lungs, and excites the energy of the ſentient principle, which by cauſing a contraction of the diaphragm, enlarges the

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(a) El. Phys. viii. 4. 18, 27, 28. *Not. ad Boer. Prælect. t.* v. *p.* 120.

the thorax, affords a free paſſage to the blood, and thus removes the uneaſy ſenſation ; when the ſtimulus no longer exiſting, the muſcles ceaſe to act. *(a)*

The queſtion under conſideration embraces two diſtinct ſubjects of inquiry ; we have in the firſt place to aſcertain the cauſe of the neceſſity for the admiſſion of freſh air into the lungs, and ſecondly, we have to inveſtigate the connexion which ſubſiſts between this cauſe and the contraction of the diaphragm. The firſt of theſe is ſo intimately connected with the opinions which we may adopt reſpecting the uſe of reſpiration, that the farther examination of it muſt be neceſſarily poſtponed to a ſubſequent part of the eſſay. I ſhall therefore conclude this chapter, with a few obſervations upon the latter part of the inquiry, viz. the nature of the connexion which ſubſiſts between the peculiar ſtate of the lungs produced by an interruption to the reſpiration, and the contraction of the diaphragm.

As the alternate motion of the thorax in reſpiration, proceeds without interruption in all the different conditions of the body, it is improper, in attempting to aſſign a cauſe for it, to have

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(a) Whytt on Vital Motions, sect. 8.

have recourſe to any effects which are produced only in extraordinary circumſtances; we cannot therefore eſtabliſh our hypotheſis, upon a ſtate of the lungs which takes place only in long continued or laborious reſpiration. On this account the firſt poſition in the hypotheſes of Haller and Whytt is inadmiſſible, ſince it ſuppoſes the exiſtence of a peculiar ſtate of the pulmonary circulation, which does not appear to take place, unleſs where the muſcles of reſpiration have been acting in the moſt violent manner. It may be doubted, whether in ordinary caſes an uneaſy feeling of ſuffocation actually occurs, as we are only ſenſible of this effect, when the lungs are compreſſed or diſtended in an unuſual degree, or when the air has been too long retained in them without alteration. Admitting however the exiſtence of this uneaſy feeling, and aſſuming it as the efficient cauſe of the ſucceeding enlargement of the thorax, we ſhall hereafter endeavour to aſſign a more probable cauſe for it than the ſtagnation of the blood in the lungs.

One of the properties which, in an eſpecial manner, diſtinguiſhes the living body from every other kind of matter, is its irritability, or the power which certain parts of it poſſeſs of contracting themſelves under peculiar circumſtances.

This power reſides in the muſcular fibres, and is never exerted unleſs when called into action by the application of a ſtimulus. Stimuli are external or internal; under the firſt denomination are comprehended a great variety of bodies which have ſcarcely any property in common, except their effect upon the muſcular fibre. The internal ſtimuli conſiſt of affections originating in the nervous ſyſtem, of the paſſions and emotions of the mind, and in a remarkable manner of the exertions of volition. The motions which reſult from theſe contractions have been variouſly arranged, either according to the cauſe producing them, or to the purpoſes which they ſerve in the animal œconomy. One of the moſt common, and at the ſame time, one of the moſt important diviſions, is into voluntary and involuntary motions, an arrangement obviouſly founded upon the ſhare which the will has in their production. There are a very conſiderable number of animal motions, which are intermediate between the two extremes; theſe are to a certain extent produced by the application of external ſtimuli, and conſequently independent of volition; yet within certain limits we have it in our power to encreaſe or reſtrain them. Animal motions which are produced by external ſtimuli, are diſtinguiſhed from each other, according to the manner in which the ſtimulus is applied ; in general the motion is excited in the part upon which the ſtimulus immediately acts, while in other inſtances, the ſtimulus is applied to a part which has little apparent connexion with that in which the motion is ultimately produced. Almoſt all the voluntary actions are of this complex deſcription, and ſome of thoſe alſo which are produced by the agency of external ſtimuli.

There is a claſs of motions of very peculiar importance in the animal œconomy, which have obtained the name of inſtinctive actions; they ſeem to originate primarily from the operation of external ſtimuli, but the effect is generally produced in a part different from that to which the ſtimulus was applied. In the firſt inſtance they appear to be totally independent of volition, though, after they have ſubſiſted for ſome time, the will acquires the power of modifying their extent and duration. The proceſs of deglutition, the motion of the eye lids, and the evacuation of the inteſtinal canal, may be adduced as inſtances of this ſpecies of animal action. Here, the animal immediately after birth, produces an effect depending upon the co-operation of a complicated ſyſtem of muſcular contractions ; the efficient cauſe, appears to be a ſtimulating body applied to a part often remotely connected with the muſcles which are excited into action, where the animal, muſt of neceſſity, be entirely ignorant of the propoſed effects, and perhaps ſcarcely conſcious of their exiſtence. *(a)*

To apply theſe obſervations to the action of the thorax: it appears, that when the blood has remained for ſome time in the pulmonary veſſels, without the acceſs of freſh air, it undergoes a change, either in its compoſition or its diſtribution, which cauſes it to act as a ſtimulus to the diaphragm, and excites this muſcle to contract. The connexion between this change in the ſtate of the blood, and the contraction of the diaphragm, we are unable to explain, and we can only recognize it from its effects; whether it be produced by an alteration in the chemical properties of the blood, or whether it depend upon an accumulation or deficiency of it in any part of the thorax, whether the action be directly upon the diaphragm or its nerves, or whether it be brought about by the intervention of any neighbouring organ, are queſtions which the preſent ſtate of our knowledge will not permit us to anſwer. But in whatever manner the contraction of

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(a) Note 21.

of the diaphragm be produced, the conſequent enlargement of the thorax admits the acceſs of air into the pulmonary veſicles; the blood is thus enabled to undergo the neceſſary change, and the ſtate which cauſed the contraction of the diaphragm no longer exiſting, this muſcle becomes relaxed, and expiration enſues. If the juſtice of theſe remarks be admitted, we may divide reſpiration into three diſtinct ſpecies, each of which originates from a different principle of action. What has been juſt deſcribed, which may be conſidered as the ordinary proceſs of reſpiration, is the firſt ſpecies: in this the effect ſeems to depend upon the action of a ſtimulus applied to an irritable part. When, however, from any accidental or morbid cauſe, the regular train of actions is interrupted or impeded, ſo as to prevent the blood from undergoing the neceſſary change in its compoſition, an uneaſy ſenſation ariſes, and we are urged by an inſtinctive feeling to encreaſe the contraction of the intercoſtals and diaphragm, and to employ the external muſcles of the thorax. Theſe motions, which produce the ſecond ſpecies of reſpiration, muſt however, be ſtill conſidered as independent of volition, becauſe they take place to the fulleſt extent in thoſe caſes where we cannot ſuppoſe the powers of the will to be exerciſed either directly, or by the intervention of aſſociation. As however we gradually advance in life, we acquire a degree of voluntary power over the muſcles which are ſubſervient to reſpiration, and can ſo far counteract the irritative, or inſtinctive motions, as very conſiderably to accelerate or retard their action. In this manner is produced the third ſpecies of reſpiration, or that which depends upon the exerciſe of volition.(a)

(a) Note 22.

PART II.

THE DIRECT EFFECTS OF RESPIRATION.

THE direct effects of respiration may be conveniently arranged under three divisions; the mechanical effects produced by the dilatation and contraction of the thorax; the change produced in the inspired air; and the alteration which the blood experiences during its passage through the capillaries of the lungs.

CHAP. I.

The Mechanical Effects produced by the Dilatation and Contraction of the Thorax.

THE proceſs by which the cavity of the thorax is alternately dilated and contracted, and a portion of the air of the atmoſphere received into and emitted from the bronchial veſſels of the lungs, has been already deſcribed. I propoſe in the preſent chapter to examine what effects will be produced upon the contiguous parts, by the perpetual change of bulk which the thorax experiences. The older phyſiologiſts, whoſe knowledge reſpecting the function of reſpiration, was for the moſt part, very limited, and who were entirely ignorant of the nature of the chemical changes produced upon the air and the blood, were led to direct their attention more particularly to its mechanical effects. The queſtion, whether the blood was tranſmitted through the lungs with more eaſe during the ſtate of inſpiration or of expiration, was minutely diſcuſſed, and a number of experiments were performed to decide upon this point; but they will be found to apply rather to what takes place in extreme caſes, than to the ordinary proceſs of reſpiration. When attempts have been made to reſolve this queſtion by a direct experiment upon a living animal, the loſs of blood, the pain which was neceſſarily inflicted, and more particularly the agitation into which the organs of reſpiration were thrown, render the reſults of comparatively little value, ſo far as they relate to the ordinary action of the thorax; though we certainly learn from them, that the blood is tranſmitted through the lungs with more facility in the extreme ſtates of inſpiration than of expiration. In an experiment performed by Dr. Hales, where a tube was inſerted into the crural artery of a horſe, the blood was evidently propelled to a greater height when the animal ſighed deeply, which muſt be attributed to the encreaſed capacity of the lungs, permitting the blood to paſs more freely through the pulmonary veſſels. *(a)* But though from this experiment, and others of a ſimilar nature related by Haller, it ſeems proved, that in theſe extraordinary caſes, the paſſage of the blood through the lungs is affected by their ſtate of diſtention, we muſt not conclude that this effect is produced in the uſual alternations of reſpiration. There are indeed ſtrong reaſons in ſupport of the contrary opinion. In the healthy ſtate of the body we reſpire about twenty times in a minute, while the average velocity of the pulſe is about eighty, ſo that the heart contracts four times during one act of reſpiration, *(b)* and conſequently it muſt receive its ſupply of blood from the lungs in all the different conditions of diſtention to which they are ſubject, yet we do not find that the pulſe is affected, either with reſpect to its ſtrength or its velocity. *(c)* It is difficult to produce any effect upon the pulſe by the moſt powerful voluntary efforts of inſpiration or of expiration, yet in theſe caſes, the cavity of the thorax certainly undergoes a much greater change in its dimenſions, than that which it experiences in its ordinary action.

The contrary doctrine was however, maintained

(a) Statical Essays, v. ii. *p.* 6. *(b) Haller,* viii. 4. 29. *Boer. Præl. t.* v. *p.* 128. *(c)* Note 23.

tained by Haller; he ſuppoſes, that during expiration, the bronchia are, to a certain degree, plaited or folded up, but that when the lungs are inflated, theſe folds are opened, and the blood veſſels rendered more eaſily permeable. *(a)* This opinion, which was generally adopted by Haller's contemporaries, *(b)* and by many of them exaggerated in a moſt extravagant degree, *(c)* ſeems to have ariſen from the following circumſtances. 1. They greatly over-rated the change of bulk which the thorax experiences in the different ſtages of reſpiration. If we average the contents of the thorax at 280 cubic inches, and the bulk of a ſingle inſpiration at 40, we ſhall have its cavity encreaſed or diminiſhed by $\frac{1}{7}$ part only; whereas the older phyſiologiſts conceived, that by every act of inſpiration, the cavity of the cheſt was doubled, quadrupled, or increaſed in even a ſtill greater proportion. *(d)* 2. In the experiments and obſervations which were made upon the dead body, they conſidered only the effects which are produced by a complete evacuation of the lungs, and then applied theſe to explain the effects of ordinary reſpiration. *(e)* 3. Haller and his friends appear to have been miſled

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(a) El. Phys. vi. 4. 10; viii. 4. 11; viii. 5. 21. *& alibi*; *notæ ad Boer. Prælect. t.* ii. *p.* 176. *(b)* Note 24. *(c) Bell's Anatomy, v.* ii. *p.* 188. *& seq.* *(d) Haller,* viii. 4. 11. *(e)* Note 25.

by the experiments which they performed upon living animals; they perceived that the tranſmiſſion of blood through the lungs was influenced by their different ſtates of diſtention, but they neglected to make the due allowance for the unnatural ſituations in which the ſubjects of theſe experiments muſt neceſſarily have been placed. Dr. Goodwyn, in order to controvert this opinion reſpecting the ſtate of the lungs after expiration, produced an artificial hydrothorax in dogs, by introducing a quantity of water between the pleuræ. He found in ſeveral inſtances, when he had filled nearly ⅓ of the cavity of the thorax, that the paſſage of the blood was not apparently retarded, though the reſpiration was rendered laborious. *(a)* Upon the whole I think there is reaſon to conclude, that in the uſual act of reſpiration, the blood is tranſmitted through the lungs at all times with nearly equal facility, and that it is only in extreme caſes that the retardation deſcribed by Haller can be ſuppoſed to take place. *(b)* If any conſiderable impediment had been oppoſed to the entrance of the blood into the pulmonary veſſels during the ſtate of expiration, it muſt neceſſarily have produced upon the pulſe a correſponding effect, which would have been perceptible to the touch. *(c)*

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(a) Goodwyn, p. 45. Note 26. *(b)* Note 27. *(c)* Note 28.

This doctrine of the older phyſiologiſts reſpecting the tranſmiſſion of the blood through the lungs, gave riſe to an opinion, which was almoſt univerſally adopted by them, that the motion of the thorax in reſpiration was an important agent in promoting the circulation of the blood. This hypotheſis will be conſidered more fully, when we come to treat of the uſes of reſpiration; I ſhall in this place only remark, that the ſame conſiderations which lead us to conclude that the lungs are nearly alike permeable to the paſſage of the blood in all the ſtages of ordinary reſpiration, muſt prove that their motion can have little or no effect in accelerating its progreſs through their veſſels.

The œſophagus paſſes through the muſcular portion of the diaphragm, and is ſo interlaced by the croſſing of its fibres, that it ſeems as if the tube muſt neceſſarily be compreſſed by their contraction. (a) Hence it has been ſuppoſed, that during inſpiration the paſſage to the ſtomach will be contracted or entirely obſtructed; but here, as in the former caſe, it is doubtful whether this effect be produced, except in the more violent efforts of inſpiration.

(a) *Haller*, viii. 1. 36. xviii. 4. 9. *Senac, Mem. Acad.* 1729. *Winslow's Anat. Sect.* 3. *Art.* 13. *Bell's Anat. v.* i. *p.* 326.

Some of the older phyſiologiſts imagined that important effects would reſult from the action of the diaphragm, upon the aorta and vena cava. The aorta, however, deſcends into the abdomen, between what are called the crura of the diaphragm, and ſeems to be ſo well protected by the arching of theſe muſcular appendages, as to be altogether defended from the effects of compreſſion. (a) There ſeems ſtill leſs probability of ſtrangulation being produced in the vena cava, as it not only pierces the diaphragm in its tendinous part, but paſſes through a large opening, the edges of which are furniſhed with a peculiar ſtructure, which would ſeem to prevent any alteration in its ſhape or dimenſions. (b) Yet notwithſtanding this ſtructure, Haller obſerved in the courſe of his experiments, that certain poſitions of the diaphragm ſo far compreſſed the vena cava, as to impede the paſſage of the blood. (c) But we may conjecture that this effect was produced by the unnatural circumſtances in which the animal was placed during the experiment; we may alſo attribute ſomething to the difficulty of making accurate obſervations upon animals ſuffer-ing extreme torture, and convulſed by the near approach of death.

(a) *Senac, & Bell, ubi supra.*

(b) *Winslow, Senac, & Bell, ubi supra.*

(c) *Haller,* vi. 4. 10; viii. 1. 36; viii. 5. 23.

The older anatomiſts aſcribed very important effects to the ſucceſſive compreſſion and relaxation of the nerves which paſs through the diaphragm, or along its ſurface. The alternate motions of the thorax, and even of the heart itſelf, were aſcribed to this circumſtance, but later obſervations have proved that it was inadequate to the propoſed effect, and have alſo pointed out cauſes, which are more agreeable to the analogy of the animal œconomy. The par vagum, and great ſympathetic nerves, both paſs through the ſmaller muſcle of the diaphragm, *(a)* and it has been conjectured, that by the alternate contractions and relaxations of this organ, they are compreſſed at regular intervals, and thus the tranſmiſſion of the nervous energy is interrupted, ſo as to produce the vermicular motions of the ſtomach and inteſtines. I have already noticed the hypotheſis of Dr. Martine, who conceived, that the compreſſion of the phrenic nerve by the lungs, when in a ſtate of inſpiration, was ſufficient to account for the ſucceeding relaxation of the diaphragm,

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(a) Haller, viii. 1. 35, *& seq. Winslow's Anat. Sect.* 6. 137, 400.

phragm, and conſequent emiſſion of air from the thorax.

The alternate enlargement and contraction of the bulk of the thorax, in conſequence of the change in the diaphragm, muſt neceſſarily produce a correſponding change in the poſition of all the abdominal viſcera. The agitation and preſſure which they will from this cauſe experience, has been generally ſuppoſed to be inſtrumental in propelling the blood along their veins; *(a)* but this effect, if it exiſt at all, has been much over-rated. From the experiments of Dr. Menzies, in which the body was immerſed in water for the purpoſe of aſcertaining the quantity of air employed in a ſingle inſpiration, it was found that the increaſe of bulk which the body experiences, is exactly equal to the volume of the air received by the lungs. *(b)* Hence we muſt conclude, that the cavity of the abdomen is at all periods of the act of reſpiration of an equal capacity, the encreaſe of bulk which the abdomen would acquire, in conſequence of the relaxation of its anterior muſcles, being exactly balanced by the contraction of the diaphragm, and conſequently the preſſure exerciſed upon the viſcera, muſt

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(a) Haller, viii. 5. 23.

(b) Menzies, p. 24, *&* *seq.*

muſt be altogether, or nearly ſimilar in all the different conditions of the thorax. But a uniform preſſure upon the veſſels can have no effect in accelerating the flow of the blood; the veins are in this caſe reduced to the ſtate of rigid tubes, and their contents can only be propelled by the momentum which they poſſeſſed previous to their arrival at this part of the circulation. An encreaſe of preſſure under theſe circumſtances, muſt rather retard than promote the flow of the blood.

This view of the ſubject is countenanced by the deficiency of valves in theſe veſſels, a circumſtance in which the veins of the abdominal viſcera differ from thoſe of moſt other parts of the body. *(a)* The veſſels which lie near the ſurface, or contiguous to the great muſcles, and which conſequently are at all times peculiarly expoſed to the effects of irregular preſſure, are plentifully furniſhed with ſemilunar valves. When, therefore, by an extraneous cauſe, the capacity of any part of a vein is diminiſhed, the blood which it contained muſt neceſſarily be driven forward towards the heart, and upon the removal of the compreſſion, the veſſel regaining its natural

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(a) Haller, ii. 2. 18, 21.

natural capacity will afford a more eaſy admiſſion to the ſucceeding column of blood. But the acceleration which is produced in this caſe, depends entirely upon the preſence of the valves; when, therefore, we find a ſyſtem of venous veſſels in which they are wanting, we may infer that the veins in queſtion are not much expoſed to the effects of preſſure, and that when preſſure is applied, it will not accelerate the motion of the fluids which they contain. *(a)*

Haller inſiſts ſtrongly upon the effects which are produced in the liver by the contraction of the diaphragm. He conceives that it is a principal agent in propelling the blood along the hepatic veins, and more particularly in preſſing the bile out of the gall bladder: he ſuppoſes that this organ is not furniſhed with any muſcular fibres, and that conſequently its contents can only be evacuated by external preſſure. There ſeems every reaſon to conclude that the effects which he deſcribes will be produced in the more violent and ſudden motions of the diaphragm, particularly thoſe which occur in the act of vomiting. *(b)*

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(a) *Haller,* iii. 2, 3; & viii. 5. 23. *Bell's Diſsections,* *v.* i. *p.* 48.

(b) *Haller,* viii. 5. 23. & xxiii. 3. 29.

The alternate action of the thorax is alſo ſuppoſed to exerciſe a conſiderable degree of compreſſion upon the lacteals, and conſequently to promote the flow of the chyle. *(a)* The remarks made above with regard to the effect of the diaphragm upon the veins of the abdominal viſcera, apply to the preſent caſe, ſo far as reſpects the uniformity of the preſſure in the ordinary act of reſpiration. But the lacteals differ from the veins of the viſcera in being furniſhed with numerous valves, *(b)* it is therefore probable that the chyle will be propelled towards the thoracic duct, in the more violent action either of the diaphragm, or the muſcles of the abdomen, and ſtill more in thoſe voluntary exertions where both theſe parts are ſtrongly contracted at the ſame time. But it is only in the more unuſual ſtate of the reſpiratory organs, that this effect is produced; in ordinary caſes it is probable, that the progreſſive motion of the chyle is effected ſolely by the muſcular action of the veſſels themſelves. The thoracic duct ſoon after its commencement, lies under what are called the crura of the diaphragm, and it is ſuppoſed by Haller, that the paſſage of the chyle along the duct is materially promoted by the contraction of this part. *(c)* But it

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(a) Senac, Mem. Acad. 1724. *(b)* Note 29.

(c) Haller, xxv. 2. 6,

it is probable that in this, as in the former inſtance, it is only in the more violent exerciſe of the reſpiratory organs that any effect can be produced upon the motion of the chyle, and that in ordinary circumſtances, the muſcularity of the veſſel is adequate to the propulſion of its contents. As a deciſive argument in favour of this opinion, it has been obſerved, that the chyle has ſtill continued its progreſſive motion along the thoracic duct after the thorax has been laid open, and conſequently the effect of external preſſure entirely removed. *(a)*

From the above remarks, we are led to conclude, that the effects produced by the dilatation and contraction of the thorax, which takes place in ordinary reſpiration, are much leſs conſiderable than was conceived by the older phyſiologiſts. In the experiments which they performed upon living animals, they neglected to make a due allowance for the unnatural ſituation in which the ſubjects were neceſſarily placed, and in thoſe which they made upon the body after death, they proceeded upon the ſuppoſition, that the moſt complete change in the capacity of the thorax, of which it is capable, was in all in-

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(a) Senac, Mem. Acad. 1729. *Cruickshanks on the Absorb. p.* 169.

ſtances produced. The experiments, therefore, exhibit the effects ariſing from a dilatation and contraction of the thorax, which can take place in the moſt extreme caſes only, and the concluſions are conſequently inapplicable to the phenomena of ordinary reſpiration.

CHAP. II.

The change produced by Respiration, in the Inspired Air.

THAT a change is produced by reſpiration upon the air received into the lungs, could ſcarcely eſcape the notice of the moſt curſory obſervers of the operations of the animal œconomy. The ancients were not unacquainted with this circumſtance, but the firſt accurate notions which we obtained reſpecting it, were deduced from the experiments of Boyle. This philoſopher, who ſo ſucceſsfully advanced the theory and practice of the various branches of experimental ſcience, was peculiarly fortunate in his reſearches into the nature and properties of the atmoſphere. He not only proved by means of the newly invented machine, the air-pump, the abſolute neceſſity of air to the ſupport of animal life, but he farther diſcovered, that the action of the lungs is quickly ſuſpended, unleſs they are furniſhed with a regular ſupply of freſh air. From this fact it was naturally concluded, that the air had undergone ſome important change during its continuance in the pulmonary veſicles, and a variety of hypotheſes and conjectures were formed to account for this alteration. The knowledge which was then obtained reſpecting the air was, however, almoſt entirely confined to its mechanical properties, ſo that the theories of reſpiration formed during this period, were neceſſarily crude and imperfect. Boyle perceived that the air in paſſing through the lungs became loaded with a quantity of aqueous vapour, and he farther ſuppoſed, that it acquired what he calls recrementitious ſteams; *(a)* but reſpecting the nature of theſe ſteams he forms no conjecture. He obſerved alſo, that the air in which an animal had reſpired for ſome time, was conſiderably diminiſhed in volume, an effect which he attributed to the loſs of part of its elaſticity or ſpring. The contemporaries of Boyle, for the moſt part, coincided with him, in his ideas reſpecting reſpiration; there were, however, ſome philoſophers who ſuppoſed, that beſides the addition of theſe vapours, the air, during its continuance in the lungs,

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(a) Boyle's Works, vol. i. *p.* 99, *&c seq.*; *vol.* iii, *p.* 371, *&c seq.*

lungs, imparted ſomething to the blood. Among theſe, the firſt in point of genius and originality was Mayow of Oxford. He inveſtigated the properties of the air, and the effects produced upon it by reſpiration, with great acuteneſs, and concluded, that a peculiar volatile ſpirit, which was one of the conſtituents of the atmoſphere, was abſorbed by the blood during its paſſage through the lungs. Borelli, *(a)* Lower, *(b)* Willis, *(c)* and others, adopted opinions in many reſpects ſimilar to that of Mayow; they imagined, that either a portion of the whole maſs of air, or ſome particular conſtituent of it, was abſorbed by the blood, and by this means converted this fluid from the venous to the arterial ſtate. But ſo little real knowledge was at this time poſſeſſed reſpecting the compoſition of the atmoſphere, that they entirely failed in their attempts to aſcertain the nature of the matter abſorbed, and their hypotheſes appeared ſo extravagant, and ſo little founded upon truth, that their doctrines fell into diſcredit, became neglected, and at length were totally forgotten. *(d)*

Dr. Hales devoted much of his attention to this ſubject, and performed many experiments with

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(a) De Motu Anim. p. 2*a prop.* 113. *(b) De Corde, p.* 159—165. *(c) Willis, pharm. Rat. p.* ii. *p.* 34. *(d)* Note 30.

with a view to illuſtrate the manner in which the air is affected by the lungs; he concludes nearly as Boyle had done, that it acquires a noxious vapour, and that its elaſticity is diminiſhed.(a) The learned Boerhaave confeſſes his inability to explain the change which the air experiences by reſpiration. (b) The opinion of Haller was not materially different from that of Boyle and Hales; he has collected all the different theories which have been advanced upon this ſubject, and after reviewing them with his accuſtomed candour and perſpicuity, he concludes that the air, when it is emitted from the pulmonary veſicles, is combined with a quantity of water, and a peculiar noxious vapour, and has its elaſticity diminiſhed. (c) Such was the imperfect ſtate of our knowledge, when Haller wrote his Elements of Phyſiology! This noble monument of induſtry and genius was ſcarcely publiſhed, when Dr. Black commenced his experiments upon fixed air, and among other intereſting diſcoveries, ſatisfactorily proved, that this peculiar gaſeous ſubſtance is generated in the lungs during reſpiration. (d) Shortly after this period, the brilliant diſcoveries of Scheele and Prieſtley made us acquainted with the nature and compoſition of the atmoſphere, and by this means effected a revolution in chemiſtry, and in all the branches of natural philoſophy connected with this ſcience. The atmoſphere, which until this period had been regarded as an homogeneous, elementary body, was diſcovered by theſe celebrated experimenters, to be compounded of two aeriform fluids, poſſeſſing diſtinct properties, and ſerving totally different purpoſes in the œconomy of nature. Theſe ſubſtances, which have ſince obtained the names of the oxygenous and the azotic gaſſes, were found to exiſt in the atmoſphere, in the conſtant proportion of about 22 to 78, *(a)* but after an animal had breathed for any length of time in the ſame quantity of air, the oxygenous part appeared to be conſiderably diminiſhed. Dr. Prieſtley repeated theſe experiments under a variety of circumſtances, and imagined, that in theſe caſes the air had undergone the ſame change in its properties, which it experiences from combuſtion, fermentation, the calcination of metals, or other operations by which the air is deprived of part of its oxygene, which, in conſequence of his peculiar theory, he ſtiles phlogiſtic proceſſes. The change

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(a) *Statical Essays, passim.* (b) *Prælect, t.* v. *p.* 169. *& seq.* (c) *Notæ ad Boer. Prælect. t.* v. *p.* 170. *El. Phys.* viii. 3. 11; viii. 5. 19, 20. (d) Note 31.

(a) Note 32.

change produced in the air by paſſing through the lungs, according to this philoſopher, conſiſted in the removal of part of the oxygene, and in the addition of phlogiſton, and a quantity of aqueous vapour. *(a)*

About a year after the publication of Dr. Prieſtley's experiments, a memoir on reſpiration appeared from the pen of the celebrated M. Lavoiſier. *(b)* After paying a tribute of reſpect to the genius of Dr. Prieſtley, he proceeds to an accurate examination of his experiments, and the concluſions which were deduced from them. He agrees with the Doctor in ſuppoſing, that the proportion of oxygene is diminiſhed in air which has been reſpired, but upon a careful analyſis of the reſidue, he finds it to differ from the air left after the calcination of metals, which is merely azotic gas, in containing a quantity of carbonic acid. He alſo obſerved, that the bulk of the air was ſomewhat diminiſhed, and we learn in general from theſe experiments, that the changes produced in air by reſpiration, conſiſt in the removal of part of the oxygene, in the addition of a quantity of carbonic acid gas, and in the diminution of its volume. He ſuppoſes that the azote is not affected by the proceſs, and that it ſerves merely to dilute the oxygenous part of the atmoſphere. (a)

(a) *Phil. Trans.* 1776; *Observ. on Air*, v. iii. *b.* 9.
(b) *Mem. Acad.* 1777.

In this paper M. Lavoiſier does not mention the aqueous vapour which is ſo evidently diſcharged from the lungs by reſpiration; it is impoſſible that it could have been overlooked by ſo accurate an obſerver; we may therefore conjecture, that he omitted to mention it, becauſe at this period he conſidered it as only diffuſed through the air expired from the lungs, by the proceſs of evaporation, and not formed in conſequence of the operation of any chemical affinities.

The concluſions of this philoſopher reſpecting the changes produced by reſpiration upon the air taken into the lungs, are for the moſt part acquieſced in by the modern phyſiologiſts, and the reſearches which have been ſince made upon this ſubject are principally directed, either to aſcertain with more preciſion the proportion of the reſpective ingredients in the air of expiration, or to frame hypotheſes to account for the operation of the lungs in effecting theſe changes.

(a) Note 33.

I ſhall defer my remarks upon the theory of reſpiration, until I have conſidered the alteration which is effected in the blood, by its paſſage through the lungs; in the remainder of this chapter I ſhall examine the experiments that have been made with a view to aſcertain with more preciſion the amount of the changes produced upon the air.

When an animal has expired in a quantity of atmoſpheric air, in conſequence of its being no longer fit for reſpiration, it is found that the whole of the oxygene is not removed from it,(a) and it appears, that independently of their bulk, different animals poſſeſs this power of abſtracting oxygene in a greater or leſs degree. It is not eaſy to deduce any general concluſions from experiments of this kind, as there are ſeveral incidental circumſtances, by which the death of the animal appears to be accelerated or retarded, but upon the whole, we are warranted to conclude from them, that the animals whoſe temperature is the highest, are the leaſt able to exiſt in air wanting its due proportion of oxygene. Accordingly birds, who poſſeſs a temperature of about 104°, have been found to expire when two-thirds of the oxygene is removed; a guinea-pig and a mouſe, whoſe temperature is about 98°, can conſume about three-fourths, while frogs are capable of living until nearly the whole of the oxygene is conſumed.(a) It muſt however be obſerved, that when animals die in conſequence of the want of freſh air, their death is not ſo much to be attributed to the deficiency of the oxygene, as to the preſence of the carbonic acid, which when it exiſts in any conſiderable quantity, exerciſes upon the body a highly noxious influence. It is therefore neceſſary to guard againſt its effects by preſenting to the air a ſubſtance which has the property of abſorbing it as rapidly as it is produced. When theſe precautions were employed, Lavoiſier found, that a guinea-pig could live without inconvenience in air which contained only one-fifteenth part of its bulk of oxygene, and when the proportion was ſtill farther diminiſhed, the only apparent effect produced, was a degree of drowſineſs in the animal.(b) As the temperature of a guinea-pig is nearly that of a man, it is probable that human life might be ſupported with air of the ſame compoſition.

(a) Note 34.

(a) *Higgins's Minutes of a Society, &c. p.* 158. *Chaptal's Chemistry, v.* i. *p,* 127.

(b) *Mem. Acad.* 1789. *p.* 574.

Another queſtion reſpecting the conſumption of oxygene, of a more intereſting nature, but of more difficult inveſtigation, is the abſolute quantity of this gas conſumed by reſpiration in a given time. The firſt calculations which were made upon this ſubject, in conſequence of the imperfect nature of the apparatus employed, and of the want of a ſufficient dexterity in the management of pneumatic experiments, were unavoidably vague and inaccurate. The difficulty was much encreaſed by a circumſtance firſt noticed by Dr. Crawford, and afterwards more fully inveſtigated by M. Jurine of Geneva, and M. Lavoiſier, that the reſpiration of the ſame animal in different ſtates of the ſyſtem, and under the operation of different external circumſtances, affects the air in very different degrees. This curious fact, which affords an inſight into ſome of the moſt important operations of the animal œconomy, muſt unavoidably produce great differences in the reſults of the beſt conducted experiments, and will render it impoſſible for us to arrive at more than an approximation to the truth. The circumſtances which have been diſcovered to influence the chemical effects of the reſpiration are, the temperature of the air reſpired, the degree of muſcular exertion, the ſtate of the digeſtive organs, and the condition of the ſyſtem as affected by fever; it is highly probable that other circumſtances will be diſcovered, by multiplying and varying our experiments upon the living body. Theſe different affections of reſpiration will undergo a farther examination in the third part of this eſſay; they are noticed in this place in order to ſhew, that the greateſt degree of certainty which we can attain upon this ſubject, is no more than an average deduced from a number of experiments, performed upon the body under the various circumſtances in which it is placed, and the conſequent changes to which it is liable.

An experiment performed by Lavoiſier, upon a guinea-pig, ſeems to have been the firſt in which a perfect apparatus, and the neceſſary degree of accuracy were employed.*(a)* The animal was confined over mercury, in a jar containing 248 cubic inches of gas, conſiſting principally of oxygene. In an hour and a quarter, the animal breathed with much difficulty, and being removed from the apparatus, the ſtate of the air was examined. Its bulk was found to be diminiſhed by eight cubic inches, and of the remaining 240 inches, 40 were abſorbed by cauſtic potaſh, and conſequently conſiſted of carbonic acid gas. Taking 100 parts of this air, theſe numbers will be as follows; the air was diminiſhed to 96·5, or by 3·5 cubic inches, and of the remainder 16·5 were converted into carbonic acid gas, and abſorbed by pot-aſh, which reduces the quantity of air to 80 cubic inches. Towards the concluſion of the experiment, the air would be neceſſarily much leſs fit for performing the functions of the lungs than the air of the atmoſphere, in conſequence of the carbonic acid gas which it contained; but as the air employed was originally much purer than the atmoſphere, the author ſuppoſes, that the quantity of oxygene deſtroyed, was probably about the ſame which would have been conſumed under the ordinary circumſtances of reſpiration,

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(a) *Mem. Acad.* 1780. *p.* 401—8.

The ſame philoſopher performed a ſecond experiment upon the ſame ſpecies of animal, with ſtill more accuracy, in which pure oxygene was employed.(a) This experiment continued during an hour and a half, and the animal being then removed from the jar, the air was analyzed as in the former caſe. 1728 cubic inches of air were found to be reduced to 1673, i. e. had ſuffered a diminution of 55 inches, cauſtic pot-aſh abſorbed about 229·5 inches, leaving a reſidue of pure oxygene.

(a) *Ann. de Chim. t.* v. *p.* 261 *& seq.*

Theſe numbers, eſtimated as in the former caſe, will be nearly as follows; 100 inches were reduced to 96·82. or by 3·18 inches, the pot-aſh abſorbed about 19 inches, reducing the whole quantity of air to 77·82 parts. The quantity of carbonic acid was here ſomewhat greater than in the former experiment, which may be attributed to the air employed being pure oxygene, and to the proceſs having been continued for a ſomewhat longer ſpace of time than in the former inſtance. Upon the whole, the reſults correſpond as nearly as can be expected, from the very delicate nature of the experiments.

Dr. Menzies firſt attempted to aſcertain the quantity of oxygene conſumed by a man in the courſe of a day. He found by experiment, that one-twentieth part of air, which had been once reſpired, is converted into carbonic acid gas; this he concludes muſt have been oxygene, as that part of the air alone is affected by reſpiration. He conceives that 720 cubic inches of air are reſpired in a minute, of which conſequently 36 will be conſumed. From theſe data he eſtimates, that in the ſpace of 24 hours 51840 cubic inches, or 17625·6 grains of oxygene, are conſumed and converted into carbonic acid gas. In this calculation ſeveral important particulars appear to have been overlooked, and accordingly it will be found to differ from the reſults of the more accurate experiments, which have been ſince performed by M. Lavoiſier, and Mr. Davy.

The experiments which were made by M. Lavoiſier, in conjunction with his friend M. Seguin, were conducted with every poſſible attention to accuracy, and with an apparatus more complete, than any which has ever been employed in phyſiological reſearches. An account of them is detailed in two papers in the memoirs of the Academy of Sciences for the years 1789 and 1790; but notwithſtanding the peculiar advantages under which they were performed, their reſults will not be found in all inſtances to coincide. M. Seguin was himſelf the ſubject of the experiments; the authors begin by pointing out the different effects which are produced by the reſpiration under the different circumſtances in which the body is placed, and they farther remark, that individuals may probably differ in the abſolute quantity of oxygene which they conſume in the ſame circumſtances. Making a due allowance for theſe variations, they conclude, that the mean conſumption of oxygene by a man during 24 hours, is ſomewhat more than 22 French cubic feet, or 46037·38 Engliſh cubic inches, a quantity a quantity of gas which will weigh 15661·66 grains troy. (a)

Lavoiſier was ſtill continuing to purſue his experiments on this ſubject, and had conſtructed a very expenſive apparatus, for aſcertaining with ſtill more preciſion, the amount of the ſeveral changes produced by reſpiration, when this great philoſopher fell a ſacrifice to the fury of Robeſpierre, and received ſentence of death. He had already performed a number of experiments with his new apparatus, and earneſtly requeſted a reſpite of a few days, in order to prepare them for publication; but his requeſt was not granted. M. De la Place, who pronounced his eulogy, has fortunately given us the moſt important reſults; they will be found to differ in ſome particulars from the former experiments, though with reſpect to the quantity of oxygene conſumed, they nearly coincide; it is ſtated that a man in 24 hours conſumes 15592·5 grains. (b)

The only experiments on this ſubject which have been performed ſince the death of Lavoiſier, are thoſe of Mr. Davy, which appear to have been executed with great accuracy. From a

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(a) Note 35. (b) *Suppl. to Enc. Brit.* v. ii, p. 594.

number of trials made upon his own reſpiration, he found, that 100 cubic inches of atmoſpheric air, after having once paſſed through the lungs, had loſt between 4 and 5 parts of oxygene, hence he calculates, that 31·6 cubic inches of oxygene are conſumed in a minute; *(a)* this will give 45504 inches in 24 hours, a quantity which will weigh 15471·36 grains. *(b)* This eſtimate coincides nearly with that of M. Lavoiſier, though it was obtained by a different proceſs and by the uſe of a different apparatus; we may therefore conclude, that between 45, and 46,000 cubic inches, or about 15500 grains, 2lbs. 8oz. troy, is the average quantity of oxygene conſumed by a man in 24 hours.

Having aſcertained the proportion of oxygene which is conſumed in reſpiration, it next remains to determine the quantity of carbonic acid gas, which is produced. It appears that Dr. Black firſt demonſtrated its exiſtence in air which was emitted from the lungs, and that M. Lavoiſier, afterwards examined it with more accuracy, and found that the air in which an animal had expired, contained about $\frac{1}{6}$ of its bulk of carbonic acid gas. *(c)* In the experiment which this philoſopher

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(a) Davy's Researches, p. 431—4. *(b)* Note 36.

(c) Mem. Acad. 1777.

loſopher performed with a more perfect apparatus, upon a guinea-pig confined in oxygene, the carbonic acid amounted to nearly $\frac{1}{3}$ of the bulk of the whole air employed, when the animal had been detained in the apparatus until the air was reduced into a ſtate, no longer fit for reſpiration. *(a)* Theſe experiments, however, only prove what proportion of carbonic acid gas, will render air incapable of ſupporting life, without acquainting us with the quantity of this gas, produced under the ordinary circumſtances of reſpiration.

M. Jurine of Geneva, appears to have been the firſt who attempted to calculate the abſolute quantity of carbonic acid formed by the reſpiration of man; he imagined that it conſtituted about $\frac{1}{10}$ part of the air emitted from the lungs. *(b)* Dr. Menzies inſtituted a ſet of experiments to diſcover the abſolute quantity generated in a given time; he infers from them, that $\frac{1}{20}$ part of air which has been once reſpired, is carbonic acid, and eſtimates, that a man in 24 hours, ſends out from the lungs 51840 cubic inches, or nearly 4lbs. troy; *(c)* but this eſtimate is probably over-rated.

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(a) Ann. de Chim. t. v. *p.* 261, *& seq.* *(b) Enc. Meth. Medicine, t.* i. *p.* 494. *(c) Menzies, p.* 50.

The circumſtances which have been already pointed out, as influencing the conſumption of oxygene, have at leaſt as powerful an effect upon the production of carbonic acid gas. Accordingly we ſhall find the calculations of the moſt accurate experimenters upon this ſubject ſo widely different from each other, that it ſeems ſcarcely poſſible to arrive at any tolerable degree of certainty.

M. M. Lavoiſier and Seguin, in their firſt memoir of 1789, eſtimate the average quantity of carbonic acid gas, formed by a man in 24 hours, at 17720 · 89 grains troy; in their ſubſequent memoir, publiſhed in the following year, this quantity is diminiſhed to 8450 · 24 grains; and in the eulogy of La Place it is ſtated, that Lavoiſier in his laſt experiments, reduced it ſtill lower to 7550 · 40 grains. Mr. Davy on the contrary, whoſe experiments ſeem to have been performed with great exactneſs, though with a leſs complicated apparatus than that employed by the French chemiſts, ſuppoſes the carbonic acid formed in 24 hours to amount to 17811 · 38 grains, *(a)* a quantity which is not very different from that firſt announced by Lavoiſier. Between ſuch oppoſing authorities, it is not eaſy to decide; upon the whole I feel inclined

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(a) Researches, p. 434.

clined to acquieſce in the experiments of Mr. Davy, and to conclude, that the quantity of carbonic acid gas, generated by a man in 24 hours, amounts to about 17800 grains or ſomewhat above 3lbs. troy.

Another of the changes produced by reſpiration upon the air, is a diminution of its volume. This circumſtance was noticed by the older phyſiologiſts, but in conſequence of the manner in which their experiments were performed, it was very conſiderably over-rated; they attributed it to a diminution of the elaſticity or ſpring of the air. (a) Lavoiſier, in the firſt memoir which he publiſhed upon reſpiration, aſcertained the degree of diminution with more accuracy, and ſtated, that air when rendered unfit to ſupport life, was reduced $\frac{1}{60}$ in bulk. (b) The experiments of Dr. Goodwyn, afforded the ſame reſult; (c) but Dr. Crawford, by ſome miſtake, was led to conclude that, when the proceſs was conducted with accuracy, there was no perceptible diminution. (d) In the account which Lavoiſier gives of the firſt experiment upon the guinea-pig, he found the diminution to amount to $\frac{1}{31}$ of the bulk of the air employed, (e) and in the ſecond ſet of experiments, the diminution was found to be $\frac{1}{32}$ part; *(a)* the greater abſorption in theſe caſes probably depending upon the greater purity of the air employed. It is ſomewhat remarkable, that in the experiments performed by Lavoiſier, in conjunction with Seguin, upon the reſpiration of man, though in other reſpects ſo remarkable for their accuracy, there is no mention made of this circumſtance, nor is it noticed by La Place, in his account of the experiments in which Lavoiſier was engaged, immediately previous to his execution. In theſe inſtances we cannot determine, whether Lavoiſier conceived that no diminution actually took place, or whether he only neglected to notice it; upon the whole, the latter appears the more probable ſuppoſition.

(a) Note 37. (b) *Mem. Acad.* 1777. (c) *Goodwyn, p.* 51. (d) *On Animal Heat. p.* 146. (e) *Mem. Acad.* 1780, *p.* 401.

The general fact of the diminution of bulk in reſpired air, has been ſince confirmed by Mr. Davy, though the exact degree of abſorption varied ſo much in his different experiments, that it is difficult from them to fix upon a quantity which may indicate the ordinary amount of this diminution. In the conſideration of this queſtion, as in the preceding one reſpecting the conſumption of oxygene, and the production of carbonic acid, there are two diſtinct objects of inquiry. We may examine the degree of diminution produced in a given quantity of air, in which an animal has been confined, until it is no longer fit for ſupporting the reſpiration; and in the ſecond place, we have to aſcertain the amount of the diminution which takes place in air that has only once paſſed through the lungs, as is the caſe in the proceſs of ordinary reſpiration. The firſt of theſe points only was examined by Lavoiſier. Mr. Davy has made experiments upon both. In air which had only once paſſed through the lungs, he found the diminution in different trials to vary from $\frac{1}{70}$ to $\frac{1}{100}$; *(a)* when he received the ſame air repeatedly into the lungs, it was found to be diminiſhed as much as $\frac{1}{18}$ of its original bulk. *(b)* The former experiments however alone indicate the effects of natural reſpiration, and taking an average of their reſults, the amount of the diminution will be about $\frac{1}{84}$ part of the whole air received into the lungs. I am not acquainted with any experiments to aſcertain the diminution of air that has been once reſpired, upon the accuracy of which we can depend, excepting thoſe of Mr. Davy.

(a) *Ann. de Chimie, t.* v. *p* 261.

Mr. Abernethy, in oppoſition to the generally received opinion, conceives that the quantity of air

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(a) Researches, p. 431—3. *(b) Do. p.* 435.

air emitted from the lungs is greater than that received by them. This ſingular hypotheſis is principally founded upon the ſuppoſition, that the carbonic acid diſcharged by reſpiration, is not generated by the union of the oxygene received by the blood as it paſſes through the lungs, with the carbone contained in this fluid, but that it is merely an exhalation from the ſurface of the veſicles. He eſtimates that 100 cubic inches received by the lungs, when diſcharged from them are augmented to 107½ inches. The diminution in the bulk of a quantity of air in which an animal has been confined, he attributes to its abſorption by veſſels with which he thinks it probable, the ſurface of the lungs is furniſhed, ſimilar to thoſe of the ſkin. (a)

If we have found it difficult to aſcertain with accuracy the amount of thoſe changes produced by reſpiration which we have hitherto examined, we ſhall probably experience ſtill more uncertainty in determining the quantity of aqueous vapour that is emitted from the lungs. This ſubſtance is from its nature leſs eaſy to collect and meaſure than a permanently elaſtic fluid, and accordingly the phyſiologiſts who have attempted to diſcover the quantity of it mixed with the air of expiration, have for the moſt part proceeded rather upon calculations derived from collateral circumſtances, than from the direct reſult of experiment.

(a) *Essays, p.* 146, 8.

The exhalation of water from the lungs in reſpiration, was a circumſtance which could not eſcape the moſt curſory obſerver; accordingly we ſhall find that it was noticed by the older phyſiologiſts, and was indeed regarded by them as one of the principal purpoſes which is ſerved by this function. (a) Dr. Hales performed many experiments for the purpoſe of aſcertaining its quantity; he contrived to paſs the air which he expired through a flaſk filled with wood-aſhes, which in conſequence of the pot-aſh contained in them, have the property of ſtrongly attracting moiſture. By obſerving the increaſe of weight which the aſhes had acquired in a given time, he eſtimates that the water emitted from the lungs in 24 hours, will amount to 9792 grs., above 20 oz. (b) The nature of his proceſs, however, did not admit of much accuracy. Dr. Menzies attempted to ſolve this problem by actually collecting in an allantoid fitted to the mouth, the water emitted from the lungs in a given time; his eſtimate is much leſs than that of Hales; he ſuppoſed that the quantity of water exhaled in 24 hours would amount to no more than 6 oz. or 2880 grains. *(a)* Mr. Abernethy, by breathing into a glaſs veſſel of a peculiar conſtruction, collected in an hour 180 grs. of water, containing, as he ſuppoſed, a quantity of mucous matter. According to his eſtimate, the quantity emitted in 24 hours, would amount to exactly 9oz. or 4320 grains, but as the ſubſtance which he obtained was not pure water, there muſt be ſome deduction made from it on this account. We are not informed what proportion the water bore to the mucus diſſolved in it. *(b)*

(a) Note 38.

(b) *Statical Essays*, v. ii. p. 322—4.

The difficulty of actually collecting and weighing the pulmonary exhalation, is probably the cauſe which induced Lavoiſier in his experiments upon reſpiration, to aſcertain its quantity by a calculation, founded upon the proportion between its conſtituent parts, compared with the compoſition of the other ſubſtances which are received into and diſcharged from the lungs. He firſt determined by direct experiment the quantity of oxygene conſumed, and of carbonic acid produced ; the compoſition of carbonic acid is known, and by comparing the oxygene which had diſappeared with the quantity which would have been neceſſary to form the acid, he found that the oxygene conſumed was more than ſufficient

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(a) Menzies, p. 54. *(b) Essays, p. 141.*

ficient to compoſe the carbonic acid which was actually produced. *(a)* He ſuppoſed that this ſuperabundant quantity of oxygene was employed in the formation of water, by uniting in the lungs with a portion of hydrogene; he eſtimates the amount of the water by knowing what quantity of it a given weight of oxygene can produce. This method employed by Lavoiſier muſt be confeſſed to be extremely ingenious, but at the ſame time, before we can depend upon its truth, we muſt be well aſſured that the ſeveral propoſitions on which it reſts, are themſelves well founded. There are, I confeſs, ſeveral reaſons which ſtrongly incline me to ſuſpect their validity, they will be conſidered in the following chapter; at preſent I ſhall only ſtate the concluſions which this diſtinguiſhed philoſopher has deduced from his calculations. *(b)*

Lavoiſier firſt formed an eſtimate of the water ſuppoſed to be generated in the lungs, from the experiments which he made upon the reſpiration of a guinea-pig confined in pure oxygene. He aſcertained, that the carbonic acid produced would have acquired for its formation, a quantity of oxygene less by 55 cubic inches, than had in fact diſappeared; this oxygene, the author adds, muſt either

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(a) Note 39. *(b)* Note 40.

either have been abſorbed, and have entered into ſome combination with the maſs of blood, or have united with a quantity of hydrogene diſcharged from the lungs, together with the carbone, and thus have produced water; he conceives that the latter ſuppoſition is the moſt probable. (a) The reaſons for this opinion are not detailed as fully as might be wiſhed, but he appears to have been influenced by the experiments, in which he conceived, that more caloric is diſengaged by the formation of a given quantity of carbonic acid in the lungs, than from the formation of the ſame quantity of carbonic acid by the combuſtion of charcoal in the calorimeter. This exceſs of carloric, he imagined was derived from the union of the hydrogenous and the oxygenous gaſes. (b) In the experiment with the guinea-pig, the carbonic acid amounted to 107 grs., the water was eſtimated at about 25 grs. Nearly the ſame train of reaſoning was purſued in the elaborate experiments upon the reſpiration of man, to which I have already referred, performed by the united efforts of Lavoiſier and Seguin. In the firſt of theſe memoirs, the quantity of water emitted from the lungs of a man in 24 hours is ſtated to be no more than 337·18 grs.; but in the 2d., where a more perfect apparatus was employed, the weight of water expired in 24 hours, is eſtimated at 11180·57 grs., nearly 2lbs. troy, a quantity very much larger than the eſtimate derived from the former experiments. In the experiments performed by Lavoiſier, of which an account is given by M. La Place, the quantity of water was ſuppoſed to be ſtill more conſiderable, even as much as 13704 grs. in the courſe of 24 hours. The proportion between the carbonic acid, and the water diſcharged by reſpiration, as deduced from theſe laſt experiments of Lavoiſier's, differs very conſiderably from that in the experiment performed upon the guinea-pig, and no leſs remarkably from each other. In the memoir of 1789, the carbonic acid was 17720·89 grs. the water only 337·18 grs. or as 1000 to 19 nearly; in the memoir of 1790, the carbonic acid was 8450·24 grs. and the water 11188·57 grs. or as 1000 to 1323; in the eulogy of La Place, the carbonic acid is 7550·40 grs. and the water 13704, or in the proportion of 1000 to 1815 nearly; from ſuch very diſcordant calculations it is impoſſible to draw any concluſions. Mr. Davy has not formed any eſtimate of the quantity of water exhaled in reſpiration.

(a) *Ann. de Chimie, t.* v. (b) *Mem. Acad.* 1789.

M. Lavoiſier in his earlieſt experiments upon reſpiration, concluded that the azote remained entirely paſſive; aud was received into and emitted from the lungs without undergoing any change; (a) to this opinion he always adhered, and it has been adopted by almoſt all the modern phyſiologiſts. Dr. Prieſtley, indeed, in his experiments, thought that there was an abſorption of azote as well as oxygene, (b) but as the apparatus which he employed was not ſuſceptible of perfect accuracy, and as his experiments were performed in the infancy of pneumatic chemiſtry, this circumſtance was generally attributed to ſome accidental cauſe. Dr. Prieſtley's ſuppoſition has, however, lately received a ſtrong confirmation from Mr. Davy, who uniformly diſcovered a deficiency of a ſmall quantity of azote in his experiments upon reſpiration; he eſtimates the volume of azotic gas abſorbed to be 5·2 cubic inches in a minute, (c) a quantity which, in 24 hours, will amount to about 2246·4 grs. or 4·68 oz. From the peculiar accuracy with which Mr. Davy's experiments appear to have been performed, I feel ſtrongly induced to acquieſce in his opinion, notwithſtanding the great authorities in favour of the oppoſite doctrine. The expreſſions which Lavoiſier employs, render it probable that he derived his notion reſpecting the inactivity of the azote rather from theory than from actual experiment, and the poſitive aſſertion of ſo diſtinguiſhed a philoſopher ſeems to have deterred ſucceeding experimenters from any farther inveſtigation of the ſubject. (a)

(a) *Mem. Acad.* 1777. (b) *Priestley's Exp. on Air*, v. iii. p. 380. (c) *Davy's Researches*, p. 434.

An opinion, directly the reverſe of Mr. Davy's, was advanced by M. Jurine; he imagines that azote is actually generated by reſpiration. (b) But an attentive review of his experiments will eaſily explain the cauſe by which he was led to form ſo ſingular a ſuppoſition. After taking a full inſpiration, he received the air from his lungs into four different veſſels, and he found, that the portion which was laſt expired, contained a greater quantity of azote than the others. He alſo found, that when the air had been retained for ſome time in the lungs, or when the ſame quantity of air had been ſeveral times reſpired, that it contained more azote, than when only taken once into the lungs and immediately diſcharged from them, as in ordinary reſpiration. But it is obvious, that in all theſe caſes, the encreaſed quantity of azote referred only to the greater proportion which it bore to the reſt of the air, and not in any degree to its abſolute volume. M. Jurine has, however, performed another experiment, which appears more direct, and had there been no ſource of inaccuracy, would

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(a) Note 41. (b) *Enc. Meth. Medecine, t.* i. *p.* 493—7.

certainly have been deciſive. He reſpired a quantity of pure oxygene, and upon examining it after it had been emitted from the lungs, he found it to be mixed both with carbonic acid and azote. But in this caſe there is every reaſon to believe, that this azote, which M. Jurine detected in the air of expiration, was derived from the contents of his lungs before the experiment. As it does not appear that he uſed any precautions to empty them before he breathed the oxygene, we may conclude that they would contain ſomewhat more than 200 cubic inches of azote, *(a)* which would immediately mix with the air inſpired, and conſequently be expelled along with it. In Lavoiſier's very accurate experiment upon the guinea-pig, where the quantity of air previouſly contained in the lungs of the animal bore only a ſmall proportion to the volume of gas in which it was immerſed, after the animal was removed from the apparatus, and the carbonic acid abſorbed by cauſtic pot-aſh, the remaining oxygene was nearly as pure, as at the commencement of the experiment. *(b)*

After having examined in ſucceſſion the different changes which are produced upon the air by

(a) Note 42. *(b) Ann. de Chim. t. v. p.* 262.

by reſpiration, I ſhall conclude this chapter by a ſummary view of theſe effects, so far as the preſent ſtate of our knowledge will enable us to form any preciſe ideas reſpecting them.

1. A quantity of oxygene is conſumed in reſpiration; in ordinary circumſtances atmoſpheric air, which has been once reſpired, loſes nearly ·04 of its bulk of oxygene; in 24 hours a man conſumes a quantity which will weigh about 2lbs. 8 oz.; ſomewhat more than 26 cubic feet.

2. A quantity of carbonic acid is generated by reſpiration; its volume is leſs than that of the oxygene abſorbed, nearly in the proportion of 37 to 45; the weight of carbonic acid formed in 24 hours is about 3lb.; a volume of about 22 cubic feet.

3. The whole volume of the air is diminiſhed by reſpiration; the degree of diminution is not very accurately aſcertained, but it may be eſtimated at about $\frac{1}{50}$ of its bulk.

4. A quantity of aqueous vapour, the amount of which is ſtill undetermined, is emitted from the lungs.

5. It is probable that a ſmall portion of azote is abſorbed, upon an average about $\frac{1}{160}$ part of the air reſpired, making in 24 hours about 4½oz. or 4 cubic feet.*(a)*

6. From the aſcertained proportion in which the oxygene and pure charcoal exiſt in carbonic acid, it appears that a greater quantity of oxygene is conſumed, than is neceſſary for the formation of the carbonic acid which is produced.*(b)*

(a) Note 43. *(b)* Note 44.

CHAP. III.

The change produced upon the blood by Reſpiration.

THE extreme vaſcularity of the lungs, and the great proportion of blood which is always preſent in them, induced even the oldeſt anatomiſts to ſuppoſe, that ſome important change was effected in this fluid by reſpiration; and the diſcovery of Hervey, which ſhowed that all the blood in the body paſſes through the lungs during the courſe of each complete circuit, ſtrongly tended to confirm this opinion. Soon after the doctrine of the circulation became generally received, the diſtinction between the arterial and the venous blood, was clearly pointed out, and it was perceived that this change is produced in the capillaries of the lungs. A number of conjectures, as may be imagined, were formed both to explain the nature of the change which takes place, and the manner in which it is effected.

A numerous and learned claſs of phyſiologiſts ſuppoſed that the alteration which the blood experiences in its paſſage through the lungs, is in a great meaſure mechanical. They conceived that the blood, while in the pulmonary veſſels, experiences a continual and violent agitation, by means of which, its particles, before looſely mixed, and conſiſting of ſeveral heterogeneous ſubſtances, are comminuted and perfectly united together, ſo that the whole maſs acquires a uniform and homogeneous conſiſtence. *(a)* Baglivi ſuppoſed that the blood was rarefied, *(b)* and Helvetius, that it was condenſed in the lungs; *(c)* Boerhaave imagined that the particles acquired that peculiar organization, which he thought eſſential to the exiſtence of perfect blood. *(d)*

Another ſet of philoſophers, in which we meet with the names of Hervey, *(e)* Boyle, *(f)* Hales and Haller, imagined that the blood, in its paſſage through the lungs, parted with ſome noxious

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(a) Note 45. *(b) Opera, p* 457. *(c) Mem. Acad.* 1718. *(d) Prælect. t.* ii. *p.* 184; 232 *& seq.* Note 46. *(e) Exerc. de Motu Cordis, p.* 232. *(f) Works, v.* i. *p.* 99. *& seq.*; *v.* iii. *p.* 371. *& seq.*

noxious or ſuperfluous matter, and with a quantity of aqueous vapour, which being mixed with the air, was removed by expiration.

A 3d claſs of phyſiologiſts were of opinion, that the change from the venous to the arterial ſtate, depended upon ſomething which the air imparted to the blood, whence it acquired its vital properties, and became adapted to the performance of its appropriate functions. None of theſe opinions is altogether correct even in the outline, and when their reſpective advocates proceeded to detail them more at length, they degenerated into mere fanciful hypotheſes. Lower had endeavoured to prove, independently of any theory, that the bright ſcarlet colour which the blood acquires in the lungs, was owing to the action of the air, and we are now ſo well aſſured of the fact, that it ſeems to us ſomewhat ſurpriſing, that his experiments ſhould have produced ſo little conviction. In order to diſprove a notion which prevailed in his time, that the red colour acquired by the blood in the lungs, depended upon an innate fire kept up in the heart, he inflated the lungs of a dog recently killed, and he found that the blood was reddened exactly in the ſame manner as during the life of the animal. Another favourite hypotheſis prevalent in his time to account for the change of colour which the blood experiences in the lungs, viz. that it depends upon the comminution of its particles, owing to the agitation and preſſure which it undergoes in this part of the circulation, he endeavoured to overthrow, by ſhewing, that the blood in the lungs of a newly killed animal would be reddened in the uſual manner, though they were kept continually dilated, and conſequently almoſt in a ſtate of reſt. As a further proof that the change of colour depends upon the action of the air, he obſerves, that when the blood is received into a baſin, the ſurface only of the craſſamentum, which is expoſed to the air, acquires the ſcarlet colour, while the other parts remain of the dark purple hue. *(a)* The experiments and reaſonings of Lower produced little conviction; the mathematicians *(b)* ſtill thought it more probable that the red colour which the blood acquires in the lungs, depends upon the compreſſion and agitation which it experiences in this organ. Haller ſtrongly oppoſed the doctrine of Lower, and conceived that the different ſhades of colour depend upon the greater or leſs proportion of red globules in the blood, or the degree of compreſſion which they ſuffer,

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(a) De Corde, p. 159—165. *(b) Pitcairne, Diſſertationes, p. 62—70.*

ſuffer, and explains the bright ſcarlet of the top of the clot, upon the ſuppoſition, that the red particles ſubſide to the lower part, in conſequence of their gravity, or are more compreſſed than thoſe at the upper ſurface. *(a)* After a long interval, the opinion of Lower was revived by Sig. Cigna of Turin; he endeavoured to prove, that the change of colour in the blood, from a purple to a ſcarlet, in all caſes depends upon the action of the air, and his experiments are ſuch as fairly to eſtabliſh the fact. They ſeem, however, to have excited little attention, and Cigna himſelf, in a ſubſequent memoir, appears almoſt inclined to deſert his former opinions. *(b)* It was at this period that Dr. Prieſtley commenced his inquiry into reſpiration; he repeated and varied the experiments of Cigna, placed his concluſions beyond the reach of doubt, and diſcloſed a new and important ſeries of facts, which ſerved as the ground-work for all the information that has been ſince obtained. The mutual action of the blood and air, which had been hitherto admitted as a plauſible ſuppoſition, was now decidedly proved; Dr. Prieſtley introduced a piece of dark-coloured craſſamentum into an

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(a) Prælect. Boer. Notæ ad, t. ii. *p.* 214. *El. Phys.* vi. 3. 17. Note 47. *(b) Priestley's Exper. on Air, v.* iii. *p.* 358—360.

inverted jar of atmoſpherical air, the blood, after ſome time, aſſumed the arterial color, and the air was found to have experienced the ſame change in its chemical properties as by reſpiration. He afterwards proceeded to examine the effects produced upon the craſſamentum by the component parts of the atmoſphere applied ſeparately, and by the other aeriform fluids which had been recently diſcovered. The purple coagulum was found to be reddened by oxygenous gas, ſtill more rapidly than by the air of the atmoſphere: while in azote, in hydrogene, and in carbonic acid, the contrary effects were produced, ſo that pieces of bright ſcarlet craſſamentum were reduced to the dull purple hue of venous blood. The concluſions from theſe experiments were highly intereſting and important: they ſhewed that the alteration of color which the blood experiences in the lungs, depends upon the oxygenous part of the atmoſphere; and reciprocally, that the change which the air undergoes when received into the lungs, depends upon the action of the blood in the pulmonary veſſels. The change which is in theſe caſes effected upon the air, Dr. Prieſtley ſuppoſed to be ſimilar to that produced by combuſtion, which, in conformity with his peculiar theory, he imagined to conſiſt in the addition of phlogiſton. He therefore

fore concluded, that the air in the lungs acquired phlogiſton from the venous blood, and that this abſtraction of phlogiſton conſtituted the principal difference between venous and arterial blood. Dr. Prieſtley carried the reſemblance between his experiments and the actual ſtate of the lungs ſtill farther, by introducing a moiſtened bladder between the craſſamentum and the air; he found that in this caſe, the ſame changes were produced as in the former inſtance, and thus obviated an objection which might have been made againſt his experiments, that the blood and the air are not actually in contact in the lungs, but are ſeparated by the membrane of the veſicles and the coats of the arteries. He likewiſe found, that the action between the air and the blood, was not interrupted by the intervention of a ſtratum of ſerum, or milk, but that when water, and ſome other fluids which he tried, were interpoſed, the change could be no longer produced.*(a)*

The train of experiment which had been opened ſo ſucceſsfully by Dr. Prieſtley, was purſued with no leſs aſſiduity by M. Lavoiſier. This philoſopher, by examining with more accuracy the ſtate of the expired air, found that it had acquired

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(a) Priestley's Exp. on Air. v. iii. *p.* 362—374. *Phil. Trans.* 1776.

acquired a great addition of carbonic acid; he had already demonſtrated, that this ſubſtance is compoſed of oxygene and carbone, and he accordingly concluded, that the air had acquired its carbone from the venous blood. In his ſubſequent experiments he ſo far modified his original opinion, as to conjecture that, in addition to the carbone, a quantity of hydrogene was likewiſe diſengaged from the venous blood, which uniting with another portion of oxygene, produced the water of expiration: according to Lavoiſier, therefore, the chemical compoſition of venous and arterial blood differed in the former poſſeſſing a greater proportion of hydrogene and carbone.

That the air in the lungs had acquired from the venous blood a quantity of carbone, was a fact no longer to be doubted, but it was ſtill uncertain in what manner this change of compoſition was effected. Either the oxygene of the air is combined directly with carbone abſtracted from the blood, and the carbonic acid is generated by their union, or a quantity of oxygene is abſorbed by the blood, and an equal quantity of ready formed carbonic acid diſcharged from it. Lavoiſier in his firſt memoir propoſes theſe two hypotheſes without deciding in favour of either of them; but in his later papers he adopts the former, and conſequently imagines, that all the changes produced upon the blood by reſpiration, are brought about in the lungs. The hypotheſis of Lavoiſier was adopted with ſome ſlight variations by moſt of his contemporaries; for the opinion of thoſe chemiſts who ſtill adhered to the doctrine of phlogiſton, as Prieſtley and Crawford, muſt be regarded as ſcarcely differing from Lavoiſier's, except in the name of the ſubſtance emitted from the venous blood, which was by one party called inflammable matter, or phlogiſton, and was by the other, with more preciſion, ſtated to be carbone and hydrogene.

In order to complete this theory, it was neceſſary to point out an adequate ſource, whence the blood may be ſupplied with this carbonaceous or inflammable matter, which is perpetually diſcharged from the lungs. Dr. Crawford was the firſt who paid much attention to this circumſtance; he propoſed the following explanation. The ſolids of the living body have a conſtant tendency to decay; their particles are continually changing, thoſe which are no longer fit for performing their functions are removed, and diſcharged from the body, while new ones are depoſited in their room. The arterial blood which is diſtributed to all parts of the body in the minute capillary veſſels, is the vehicle by means of which this operation is performed; it conveys the nutritious matter to the different parts of the body, and depoſits it in ſuch a manner as to repair the neceſſary waſte, while at the ſame time it receives the putreſcent particles, which are become uſeleſs or noxious to the ſyſtem, and carries them to the lungs, where they are united to oxygene, and diſcharged, together with the air of expiration. It is to the addition of this extraneous matter that Dr. Crawford attributed the change from arterial to venous blood, and by the removal of it in the lungs, the blood, he imagined, is brought back to the arterial ſtate. *(a)* The hypotheſis of Dr. Crawford, reſpecting the origin of the combuſtible matter, was generally received by the phyſiologiſts who adopted Lavoiſier's method of explaining the changes produced by reſpiration. This philoſopher himſelf does not indeed appear to have paid any particular attention to the ſubject; it may be inferred from ſome of his expreſſions, that at one period he inclined to the opinion of Dr. Crawford, but in his later memoirs he ſeems to conſider the products of digeſtion as the immediate ſource of the

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(a) Obſerv. on Animal heat, p. 150. *& ſeq.* Note 48.

the matter conſumed in reſpiration. He however ſtill ſuppoſes, that the changes are effected in the lungs, and it is therefore obvious that his theory muſt be imperfect, as the blood becomes completely venalized before it receives the contents of the thoracic duct.

This theory of reſpiration, though certainly ſimple and ingenious, was, however, found to be encumbered with many difficulties, for, independent of other objections, Dr. Crawford's explanation of the manner in which the blood acquires the ſupply of inflammable matter, is by no means ſatisfactory. It is indeed admitted, that the particles of which the ſolids are compoſed are perpetually changing, but it is contrary to all analogy to ſuppoſe, that the arteries are the inſtruments by which this change is effected. The body is provided with a diſtinct ſet of veſſels, which from their office are called the abſorbents, whoſe appropriate function it is to remove all ſuperfluous matter. There can ſcarcely be a doubt, but that it is by the action of the abſorbents, that the uſeleſs particles are removed, and theſe veſſels have no communication with the ſanguiferous ſyſtem, except by the intervention of the thoracic duct, which receives all the ſubſtances abſorbed, and pours them into the left ſubclavian vein. There is no paſſage hitherto diſcovered, by which the blood can receive any thing *ab extra*, from the time it leaves the heart, until it arrives at the mouth of the thoracic duct, when the circuit is nearly concluded, and the blood has already become completely venalized.

A ſecond objection againſt this hypotheſis, perhaps no leſs deciſive, was derived from a circumſtance which had been not unfrequently obſerved, that on ſome occaſions the blood is converted to the venous ſtate, while it ſtill continues in the great trunks of the arteries. This change has been known to take place during ſurgical operations, in which the courſe of the blood along the arteries had been intentionally ſtopped, and alſo in caſes where an extraneous, morbid body had preſſed upon the veſſels, and had prevented them from tranſmitting their contents in the uſual manner. Where the tourniquet has been applied for any length of time to the trunk of a great artery, the blood which firſt flows through the veſſel when the inſtrument is removed, is ſometimes obſerved to be of the venous colour. Mr. Hunter remarks, that extravaſated blood is in all caſes of a dark purple colour, though there is every reaſon to ſuppoſe, that it often proceeds from the rupture of an artery. He punctured the femoral artery of a dog, and afterwards carefully excluded the air from the orifice, a tumor was formed in the adjoining cellular membrane, this he opened after ſome time, and the blood which it contained was found to exhibit all the venous characters. He then proceeded to perform the following ſtill more direct and deciſive experiment; he laid bare the carotid artery of a dog and paſsed round it two ligatures, leaving between them an interval of two inches. After ſome hours he pierced the part of the veſſel between the ligatures, and he found it to contain dark purple blood.(a) In theſe inſtances it may be inferred, that the change which the blood experienced could not depend upon any ſubſtances either received or diſcharged, but upon an action which took place among the bodies already contained in it. It is indeed admitted, that the capillary arteries of the lungs poſſeſs the power of tranſmitting through their coats, various ſubſtances, which cauſe the mutual changes produced in the air and the blood; but the difference in the thickneſs and texture of the branches of the aortic ſyſtem, and the minute capillaries in the lungs, is a ſufficient reaſon for ſuppoſing that

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(a) *Obſerv. on the blood, p. 65--7.*

their contents cannot be affected in the ſame manner by external agents. The capillaries of the lungs, are moreover expoſed to the action of the oxygene of the inſpired air, whereas the arteries in the other parts of the body, independent of the thickneſs of their coats, are not expoſed to any external agent, which can be ſuppoſed to have the power of affecting the compoſition of the blood which they contain. It appears indeed, that the abſence of oxygene is a more important circumſtance than the mere thickneſs of the coats; as we learn from the experiments of Dr. Goodwyn, that the colour of the blood in the ſmall veins of the neck of a rabbit, was in ſome meaſure brightened, by directing a ſtream of oxygene againſt the outer ſurface of the veſſels, when the cellular ſubſtance was carefully removed.(a)

As it appeared therefore from theſe circumſtances, that blood may be converted from the arterial into the venous ſtate without the addition of any extraneous ſubſtance, it became neceſſary to abandon the theory of reſpiration which attributed this change to the abſorption of inflammable matter, during the aortic circulation, and to form one in which the alteration might be produced by the mutual action of the conſtituent parts of the blood upon each other. *(a)* This was accordingly done by M. La Grange, who propoſed a new hypotheſis, which M. Haſſenfratz adopted, and illuſtrated by ſeveral important obſervations and ingenious experiments. *(b)* The theory of M. La Grange, has been in ſome reſpects modified by Mr. Allen, *(c)* and with his improvements, appears to afford an eaſy explanation of every phenomenon.

(a) *Goodwyn, p.* 63.

According to this hypotheſis, the blood in its paſſage through the capillaries of the lungs, abſorbs oxygene, which is looſely united to the whole maſs of fluid; by this union its colour is changed from a deep purple to a bright ſcarlet. The oxidated blood is then carried along the arteries; in the courſe of the circulation, the oxygene leaves the whole maſs of blood, and forms an intimate union with a part of the hydrogene and carbone contained in it, by this operation it loſes its bright colour and aſſumes the venous appearance. This portion of hydrogene and carbone, reduced to the ſtate of an oxide, is then carried along the veins, until it arrives at

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(a) Note 49. (b) *Ann. de Chim. t.* ix.
(c) *De la Rive de Cal. Anim. p.* 35.

the lungs, where, after being united with an additional quantity of oxygene, it is diſcharged from the blood, and forms the carbonic acid, and the aqueous vapour which are found to exiſt in the air of expiration. According to this hypotheſis, venous and arterial blood differ only in the arrangement of their component parts, in the latter caſe the oxygene is looſely combined with the whole maſs, whereas in the former, it is in cloſe union with a portion of hydrogene and carbone only. As a confirmation of the aſſumed principle upon which this hypotheſis is founded, M. Haſſenfratz adduces an experiment performed by M. Fourcroy, in which venous blood mixed with oxygene immediately acquired the ſcarlet colour, but after ſome time, though it was ſtill expoſed to its action, it again became purple, *(a)* owing, as he conjectured, to a more intimate union, which gradually took place between the oxygene and ſome part of the blood. Dr. Prieſtley had ſeveral years before obſerved, that arterial blood was rendered purple by expoſure to azote, or to any of the gaſes which did not contain oxygene; in this caſe it may be inferred, that the gas attracted the oxygene from the maſs of blood with which it was looſely combined, for if the oxygene

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(a) Ann. de Chim. t. vii. *p.* 148.

oxygene had already been united to the carbone and hydrogene, ſo as to form carbonic acid and water, it is not probable that the mere expoſure to azote, could have decompoſed theſe ſubſtances.

The difference between the arterial and the venous blood, according to this hypotheſis, being ſuppoſed to conſiſt principally in the more or leſs intimate combination of the oxygene, M. Haſſenfratz propoſed to obſerve the effects which would be produced, by adding to the blood oxygene in a condenſed ſtate; he employed for this purpoſe the oxygenated muriatic acid. A quantity of it was accordingly poured into ſome blood, when the fluid acquired a deep and almoſt black colour. That this colour depended upon the oxygene, and not upon the muriatic acid, he proved by performing a ſecond experiment, in which he uſed the common muriatic acid; the blood in this caſe was merely rendered brown. From the effects of the oxygenated acid, he inferred, that the oxygene, being preſented to the blood in a condenſed ſtate, had immediately formed a union with the hydrogene and carbone, and had conſequently produced the ſame change inſtantaneouſly, which in ordinary circumſtances, is only brought about gradually during the courſe of the circulation. M. Haſſenfratz afterwards filled a number of tubes with arterial blood, and ſealed them hermetically; after ſome time the blood became purple, and acquired the venous appearance. The ſame action appears to have taken place in this inſtance, between the conſtituent parts of the blood, which is produced in the capillaries of the aortic ſyſtem, and it was effected in a ſituation where nothing could be abſorbed by the blood or diſcharged from it. The oxygene which had been previouſly diſſolved in the whole maſs, and had imparted to it the bright ſcarlet colour, afterwards entered into a union with part of its hydrogene and carbone; the colour was changed to a deep purple, and from the ſituation in which the blood was placed, it had not the power of diſcharging the oxidated ſubſtances which it contained, or of attracting a freſh ſupply of oxygene.

Theſe experiments certainly afford a ſtrong confirmation of the truth of M. La Grange's hypotheſis, and indeed they may be conſidered as demonſtrating that a change in the blood, ſimilar to that which it undergoes in the courſe of the aortic circulation, may be effected merely by a different arrangement of its conſtituent parts. Another circumſtance in which this hypotheſis poſſeſſes a moſt decided ſuperiority over the former, though not noticed by La Grange and Haſſenfratz, is the facility with which it accounts for the introduction into the blood of a regular ſupply of inflammable matter. According to the former theory, the change from the venous to the arterial ſtate of the blood, depended upon the diſcharge of the ſuperfluous inflammable matter, a proceſs effected in the capillaries of the lungs; the blood was therefore ſuppoſed to return to the right ſide of the heart deprived of this ſubſtance, but in paſſing along the minute arterial branches, it acquired ab extra the hydro-carbonous matter, and was thus brought back to the venous ſtate: I have already alledged the objections againſt this ſuppoſition. In the hypotheſis propoſed by M. La Grange, we may conceive the inflammable matter to enter the blood during any part of the circulation, as the different ſtates which this fluid aſſumes, depend not upon the abſolute quantity of the ſubſtances which it contains, but upon the ſtate of their combination. The nature of the inflammable matter of the blood will be conſidered more fully hereafter, at preſent I ſhall merely ſtate, that the obvious ſource of ſupply conſiſts in the contents of the thoracic duct, which are poured into the left ſubclavian vein.(a)

(a) *Skey de mat. combust. sang.*

M. M. La Grange and Haſſenfratz, in conformity with the opinion of M. Lavoiſier, that the oxygenous part of the atmoſphere only was employed in reſpiration, ſuppoſed that this gas alone is abſorbed in the lungs. The contrary opinion is, however, embraced by Mr. Davy. He obſerves, that from the experiments of Prieſtley and Cigna it appears, that venous blood can become florid through a ſtratum of ſerum, and that conſequently either the whole of the air of the atmoſphere, or the oxygenous part of it, muſt be diſſolved by the ſerum before it can arrive at the red particles, ſo as to change their colour. He thinks it probable that the whole air is abſorbed by the ſerum, and that in this condenſed ſtate, it is decompoſed by the affinity which ſubſiſts between oxygene and the red globules. A small portion of the azote is alſo retained by the blood, but the greateſt part of it is liberated without undergoing any change.*(a)*

Dr. Thomſon alſo ſuppoſes, that the atmoſpheric air in its whole ſubſtance is abſorbed, but that the greateſt part of the azote is again diſcharged. In favour of this ſuppoſition, he urges the experiments of Dr. Prieſtley, and Mr. Davy,

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(a) Researches, p. 447.

Davy, in which a ſmall quantity of azote is conſumed in reſpiration, and alſo the experiments of the latter upon the reſpiration of the nitrous oxide. As in this caſe the air of expiration conſiſts of a part only of the gas inſpired, mixed with a quantity of azote, it may be inferred, that the nitrous oxide is decompoſed by the blood, a proceſs which both Mr. Davy, and Dr. Thomſon conceive, can only be brought about by its having been previouſly diſſolved in this fluid. *(a)* But the ſtate of combination in which the oxygene and azote exiſt in the air of the atmoſphere, and in the nitrous oxide, differs ſo conſiderably, that even admitting the abſorption of this latter compound by the blood, we ſhall not be authorized to infer, that the ſame action will take place with reſpect to the former. Upon the whole I think it probable, that the blood exerciſes its attraction upon the oxygene alone, but that in conſequence of the powerful affinity which a large maſs of one of theſe gaſes poſſeſſes for a ſmall quantity of the other, the oxygene which is abſorbed by the blood, ſtill retains a minute portion of azote in combination. *(b)*

It appears then that the change produced in the

(a) Davy's Researches, loc. cit. Thomson's Chemistry, v. iv. *p.* 492. Note 50.

(b) Note 51.

the compoſition of the blood, during its paſſage through the lungs is two-fold; a portion of inflammable matter is diſcharged from it, and a quantity of oxygene is abſorbed. It ſeems upon the whole probable, that theſe changes are contemporary, and that the abſorption of oxygene, and the diſcharge of inflammable matter, at leaſt in ordinary caſes, proceed in exact proportion to each other. The immediate effect upon the blood is to alter its colour from a deep purple to a bright ſcarlet. M. Haſſenfratz's experiment, in which arterial blood aſſumed the purple hue, when encloſed in tubes hermetically ſealed, *(a)* favours the ſuppoſition, that this change of colour is more influenced by the preſence of the oxygene than of the inflammable matter.

As the blood is a heterogeneous fluid, compoſed of ſeveral ſubſtances, which are looſely connected together, and poſſeſs different chemical properties, it has been made a ſubject of inquiry, upon which of its conſtituents is the action of the air more immediately exerciſed. Of the two parts into which the blood ſeparates by its ſpontaneous coagulation, the craſſamentum and the ſerum, the former only has been made the ſubject of experiment,

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(a) *Ann. de Chim. t.* ix. *p.* 269.

periment, and has been found to effect a diminution of oxygene, and a production of carbonic acid gas in the ſame manner, as when the entire maſs of blood is expoſed to the action of the air. From this circumſtance, and alſo from the apparent ſimilarity of the ſerum to other animal ſubſtances which are not known to have any ſpecific action upon the air, it is inferred, that the craſſamentum is the immediate agent in producing the change which is brought about in the proceſs of reſpiration. The craſſamentum itſelf is a compound body, conſiſting of fibrine and the red particles, the former of theſe ſubſtances appears to poſſeſs the ſame chemical properties with the muſcular fibre, whereas the red particles are bodies of a peculiar organization, they give the blood its colour, and by the different ſhades which they aſſume, afford the principal indication of the approaches which it makes to the venous and arterial ſtates. On this account it has been preſumed, that the power of attracting the oxygene from the atmoſphere, reſides in the red particles. The nature, and even the form, of theſe particles is ſtill involved in much obſcurity; there is perhaps no ſubject in the whole range of phyſiology, reſpecting which more contradictory opinions have been advanced.(a) It has, however, been long known that they contain a quantity of iron, and from ſome late experiments performed by M. M. Fourcroy and Vaquelin,(a) the iron appears to be united to the phoſphoric acid, the compound ſtill retaining an exceſs of the metal in an oxidated ſtate. The other component parts of the red globules have been examined merely by the vague method of deſtructive diſtillation, from which it can only be aſcertained, that they contain the ſame chemical elements with animal ſubſtances in general. The experiments of M. M. Fourcroy and Vaquelin render it probable, that the colour of the red particles depends upon the phoſphate of iron, but it is ſtill uncertain, whether this metallic ſalt poſſeſſes the property of attracting the oxygene from the atmoſphere, or whether this operation be not rather effected by the red particles in their whole ſubſtance. *(b)*

(a) Note 52.

Lavoiſier was the firſt philoſopher, who formed any accurate idea reſpecting the nature of the inflammable matter diſcharged from the blood. In his firſt memoir in 1777, he conſiders it as compoſed entirely of carbone, but he afterwards conceived that a quantity of hydrogene was alſo diſengaged from the blood, and this opinion has been adopted both by the French and Engliſh phyſiologiſts. The firſt of theſe effects, the diſcharge of carbone from the blood, appears to be ſufficiently eſtabliſhed; a quantity of carbonic acid is found in the air after expiration, which did not previouſly exiſt in it, while the air is found to be deprived of part of its oxygene. Carbonic acid is aſcertained to be a compound of oxygene and carbone, and conſequently the oxygene which has diſappeared, is ſuppoſed to be converted into carbonic acid, by the addition of a quantity of carbone which has been abſtracted from the blood. (a).

(a) *Systeme des con. chim. t.* ix. *p.* 152. (b) Note 53.

The proof of the diſcharge of hydrogene from the blood is leſs direct. It has been ſuppoſed, in the ſame manner with the carbone, to be emitted in combination with oxygene, and it is indeed true, that a quantity of water, the ſubſtance produced by the union of hydrogene and oxygene, is expelled along with the air of expiration. But we may point out another origin for the aqueous vapour without having recourſe to the hypotheſis of its generation, by the direct union of hydrogene and oxygene, either in the lungs, or in the courſe of the circulation. The inner ſurface of the bronchia and air veſicles, is lined with a copious ſecretion of a mucous matter, and as the lungs are continually kept at a temperature of 98°, it may be ſuſpected that the water exhaled from them proceeds merely from the evaporation of part of this fluid. As, however, the contrary opinion is generally received, and is countenanced by the higheſt authorities, it will be proper to examine upon what foundation it is ſupported.

(a) Note 54.

It appears that the diſcharge of hydrogene from the lungs was firſt ſuſpected by Lavoiſier, in conſequence of the experiment which he made upon the reſpiration of the guinea-pig in oxygene. *(a)* He was induced to form this conjecture by obſerving, that a greater quantity of oxygene had diſappeared than what was ſufficient to compoſe the carbonic acid which was produced, and he conſidered its union with hydrogene, and the conſequent production of water, as the moſt probable method in which the ſuperabundant oxygene could be employed. M. Lavoiſier had already, in a former ſet of experiments, noticed, that ſomewhat more caloric was diſengaged by the proceſs of reſpiration than could be aſcribed to the formation of the carbonic acid emitted from the lungs,

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(a) Ann. de Chim. t. v. *p.* 265; *and t.* xxi. *p.* 227. *Mem. Soc. Roy. Med.* 1782, 1783, *p.* 574.

lungs,*(a)* and this difficulty he ſuppoſed might alſo be removed, by admitting the formation of a quantity of water, a proceſs in which, as well as in the generation of carbonic acid, caloric is evolved. It is evident, that the firſt of theſe arguments for the formation of water in the proceſs of reſpiration, depends, in a great meaſure, upon the truth of the peculiar theory advanced by M. Lavoiſier, which ſuppoſes that the oxygene attracts the inflammable matter of the blood during its paſſage through the lungs, without entering into that fluid. But according to the hypotheſis advanced by M M. La Grange and Haſſenfratz, the whole of the oxygene employed in reſpiration is conceived to be in the firſt inſtance abſorbed by the blood, and there is no reaſon to conclude, that the exact quantity of this gas, which is received into the ſyſtem by the lungs, is always diſcharged by the ſame channel. Even if we were to admit this to be the caſe, it would ſtill remain to be proved, that the carbone which enters into the compoſition of the carbonic acid, had previouſly exiſted in that ſtate of combination which conſtitutes pure charcoal.*(b)* This ſubſtance was conceived by Lavoiſier to be an elementary body, and his calculations were formed upon

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(a) *Mem. Acad.* 1780, *p.* 405. 1789. *p.* 569. *(b)* Note 55.

upon this ſuppoſition; but the experiments of Guyton and Tennant, have demonſtrated, that what was formerly conſidered as pure carbone, already contains ·36 of oxygene, and that conſequently, carbonic acid conſiſts of a greater proportion of oxygene than Lavoiſier ſuppoſed. It is however unneceſſary to purſue this calculation, as we are not aſſured of the truth of the previous ſteps upon which the hypotheſis reſts.

With reſpect to the want of correſpondence between the quantity of caloric evolved in the lungs, and the carbonic acid generated, although we might endeavour to remove this difficulty, by taking into account the caloric diſengaged during the formation of water, provided this circumſtance were deciſively proved by other evidence, yet it muſt be confeſſed, that the experiments reſpecting animal heat, and the apparatus employed for meaſuring its exact quantity, are of too delicate a nature to ſerve as a proof of the fact.

In addition to theſe arguments, which were urged by M. Lavoiſier, to prove the generation of water in reſpiration, M. Seguin adduces the experiments of Cigna, Prieſtley, and Hamilton, where the ſcarlet colour of the arterial blood was rendered purple by expoſure to hydrogenous gas. The hydrogene, he ſuppoſes, is in this caſe abſorbed by the blood, and he concludes, that if the abſorption of hydrogene can convert arterial into venous blood, the diſcharge of this body muſt bring it back to the arterial ſtate. *(a)* I think it a ſufficient reply to this argument to remark, that the ſame change in the colour of arterial blood is brought about by the application of the azotic and carbonic acid gaſes, and even by placing it in vacuo. *(b)*

From this view of the arguments which have been adduced to prove the generation of water in the lungs, and at the ſame time from a conſideration of the more obvious ſource of the aqueous exhalation, which is furniſhed by the moiſture always exiſting on their ſurface, I do not heſitate to conclude, againſt the authority of Lavoiſier, *(c)* that the diſcharge of hydrogene from the blood has been admitted without ſufficient evidence, and that at preſent we have no proof of the emiſſion of any ſubſtance from this fluid, depending upon the effects of reſpiration, except carbone. *(d)* I regard the water of the pulmonary exhalation as produced by evaporation

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(a) Ann. de Chim. t. xxi. *p* 225. *& seq.* *(b) Prieſtley's exper. on Air, v.* iii. *p* 364. *(c)* Note 56. *(d)* Note 57.

from the mucous fluid which lines the inner ſurface of the bronchia and veſicles, and which is ſeparated from the blood by ſecretion; the water that is contained in the blood mixed with the ſerum, is poured into it along with the contents of the thoracic duct, which are at all times largely diluted with this fluid. *(a)*

The arterial has been obſerved to differ from the venous blood, in containing a leſs proportion of ſerum and water, and this difference has been attributed to the operation of the lungs; *(b)* but it may, with more propriety, be aſcribed to the effects of ſecretion and tranſpiration. It is probable that the blood in its paſſage along the capillary arteries, is continually parting with its fibrine, in order to repair the waſte which the muſcular fibres experience, and the proportion of its ſerum will be conſequently increaſed when it arrives at the veins. On the other hand, during its paſſage through the minute arteries of the lungs, a quantity of the water which it contained is employed in forming the mucus which lines the bronchia and veſicles, and in ſupporting the pulmonary tranſpiration. In the mean time the blood has received a freſh ſupply of chyle, which is

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(a) Note 58. *(b) Richerand, p.* 133.

is the immediate ſource whence the fibrine is produced.

The hypotheſis advanced by M. Cuvier, *(a)* which ſuppoſes that reſpiration aſſiſts in the converſion of chyle into fibrine, appears to be better founded. We learn from accurate analyſis, that there is a greater proportion of azote, and conſequently a ſmaller proportion of carbone, in fibrine than in the chyle, the ſubſtance from which the fibrine is formed, ſo that in order to produce this change, it is neceſſary to remove from the chyle a part of its carbone; but this we have already found to be one of the principal effects produced by the proceſs of reſpiration. *(b)*

The thoracic duct pours its contents into the left ſubclavian vein, at a little diſtance from the termination of the venous ſyſtem in the right ſide of the heart. It has been obſerved, that when the blood arrives at this part of the circulation, the chyle ſtill exiſts in a ſtate of imperfect mixture with this fluid, but that after it has completed its paſſage through the lungs, the globules of chyle are no longer viſible, and the whole forms one uniform maſs. This intimate mixture

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(a) Leçons d'anat. comp. t. i. p. 92. *(b)* Note 59.

of the chyle with the other parts of the blood, has been conſidered as an effect of the reſpiration, but I think it more probable, that it is merely a mechanical operation, produced by the tranſmiſſion of the blood through the minute network of veſſels which compoſes the termination of the pulmonary veins, and the commencement of the correſponding arteries.

The diſcovery of Dr. Black, that bodies which indicate the ſame temperature, are frequently combined with different quantities of caloric, perhaps the moſt valuable of all the modern diſcoveries in chemiſtry, opened a path to the explanation of the ſingular phenomena of animal heat. This philoſopher had previouſly examined the change effected in the air by reſpiration, and had found it to be in many reſpects ſimilar to that produced by combuſtion; he accordingly inferred, that in the ſame manner as in the burning of an inflammable body, an extrication of caloric muſt take place in the lungs. This hypotheſis reſpecting the origin of animal heat was, however, advanced by Dr. Black, only as a plauſible ſpeculation, and remained open to many objections, when Dr. Crawford turned his attention to the ſubject. He performed many elaborate experiments to determine the ſpecific heat of a variety of different ſubſtances, and purſuing the analogy which had been pointed out by Dr. Black, between the effects of combuſtion and reſpiration, he particularly examined the ſtate of the blood before and after it had paſſed through the lungs. The reſult of his labours was the diſcovery of the important fact, that the blood has its capacity for heat increaſed by its converſion from the venous to the arterial ſtate, in conſequence of the change produced in it by the action of the air. He determined the capacity of venous and arterial blood to be in the proportion of 10 to 11½.(a)

As the production of animal heat appears to be one of the moſt important uſes of reſpiration, I ſhall, in adherence to my propoſed plan, defer to the 4th diviſion of this eſſay, the farther conſideration of this curious part of the animal conſtitution. I ſhall then examine at ſome length the theory of animal heat as originally propoſed by Dr. Crawford, together with the various modifications which it has received from the ingenuity of thoſe phyſiologiſts who have ſtill more recently inveſtigated the ſubject.

We have now reviewed in ſucceſſion the

(a) *Obſervations on Animal heat, p. 277.*

changes which are ſuppoſed to be effected in the blood by reſpiration. It will be perceived, that notwithſtanding the diſcovery of many important facts, the ſubject ſtill remains involved in much obſcurity. This muſt be, in a great meaſure, attributed to the difficulty of performing experiments upon a ſubſtance like the blood, compoſed of a number of different ingredients, connected together by a complicated ſyſtem of affinities, which is liable to be diſturbed by the operation of almoſt every external body. The difference between the arterial and the venous blood is rather inferred from the compariſon of a number of obſervations, that have been made upon the body in its different ſtates, than aſcertained by the direct reſult of experiment, and conſequently the information which we have acquired upon this ſubject, is ſtill vague and imperfect. There are, however, a few points, which the labours of the modern phyſiologiſts ſeem to have clearly eſtabliſhed, and from the progreſſive ſtate of chemical ſcience, we have reaſon to expect that their number will gradually increaſe.

The preſent ſtate of our knowledge, reſpecting the change produced on the blood by reſpiration, may, I think, be comprized in the following propoſitions.

1. The blood which is expelled from the right ventricle of the heart, into the pulmonary artery, is of a dull purple colour; during its paſſage through the capillaries of the lungs, its colour is converted into a bright ſcarlet, in which ſtate it is returned to the left auricle of the heart.

2. The change of colour is effected by the oxygenous part of the atmoſpheric air, which is received into the veſicles of the lungs.

3. The blood in paſſing through the lungs, emits a quantity of carbone, which is expired in combination with oxygene, under the form of carbonic acid gas.

4. The blood poſſeſſes an attraction for oxygene, and during its paſſage along the capillaries of the lungs, it abſorbs a quantity of this ſubſtance.

5. To one of theſe circumſtances, viz. the emiſſion of carbone, and the abſorption of oxygene, or to the combination of the two, is to be aſcribed the change from the purple to the ſcarlet colour, which the blood undergoes during its paſſage through the lungs.

6. The ſame change of colour is produced, when a portion of the clot of venous blood, is expoſed to the action of oxygenous gas, and the proceſs is not interrupted by the interpoſition of a moiſtened membrane, or a ſtratum of ſerum, between the craſſamentum and the air.

7. When arterial blood is expoſed to the azotic, hydrogenous, or carbonic acid gaſes, or placed in the vacuum of an air pump, it aſſumes the purple hue of venous blood.

8. The oxygene, which is, in the firſt inſtance, looſely combined with the whole maſs of blood, during the courſe of the circulation, gradually enters into a more intimate union with a part of its carbone, and forms with it an oxide. When this oxide is brought back to the lungs, it unites with an additional quantity of oxygene, ſo as to form carbonic acid, and is then removed with the air of expiration.

9. The blood receives its ſupply of carbonaceous matter from the contents of the thoracic duct, which are poured into the left ſubclavian vein.

10. It has been ſuppoſed that, beſides the carbone, a quantity of hydrogene is diſcharged from the blood by the lungs, but, this opinion appears to have been adopted, without ſufficient foundation.

11. In conſequence of the abſtraction of a quantity of carbone from the blood, the proportion of azote in the reſiduum is augmented; it is alſo probable, that a ſmall quantity of azote is abſorbed by the blood, immediately from the atmoſphere.

12. When blood is converted from the venous to the arterial ſtate, its capacity for heat becomes increaſed; the ſpecific heats of venous and arterial blood are in the proportions of 10 to 11½.

13. The blood, in conſequence of its paſſage through the lungs, appears to have its conſtituent parts more uniformly mixed together, but this effect is to be attributed merely to the agitation and preſſure which it experiences during its courſe through the minute and convoluted net-work of veſſels which compoſe the capillaries of the lungs.

14. Beſides the circumſtances which have been enumerated, the arterial differs from the venous blood, in conſequence of the ſubſtances ſeparated from it to form the different ſecretions; but theſe changes are not to be attributed to the effects of reſpiration, and it is abſolutely impoſſible to appreciate their amount.

15. A conſiderable quantity of aqueous vapour is emitted from the blood, but this is the effect of tranſpiration, and conſiſts of water which had been poured into the blood ready formed by the thoracic duct, along with the other products of digeſtion and abſorption.

CHAP. IV.

On the reſpiration of the different Gaſes.

IT is probable that no aeriform fluid, except the compound of oxygene and azote which exiſts in the atmoſphere, is adapted to the permanent ſupport of life. Of the other gaſes, there are ſome, which on account of their irritating nature, it is abſolutely impoſſible to receive into the trachea; theſe properly conſtitute the non-reſpirable gaſes. There are, however, others which it is poſſible to inſpire, though their employment is followed ſooner or later by the extinction of life. I ſhall detail ſome of the principal experiments that have been performed on this ſubject, as the nature of the change produced upon the blood by common reſpiration may be in ſome meaſure illuſtrated, by obſerving the effects which follow the uſe of the other gaſeous bodies.

Soon after the diſcovery of the appropriate power which the oxygenous part of the atmoſphere poſſeſſes, of ſupporting animal life, ſeveral trials were made of the effects which would reſult from breathing it in an unmixed ſtate. The accounts given by thoſe who were the ſubjects of theſe trials were various; Dr. Prieſtley, who firſt made the experiment, conceived that he felt an agreeable lightneſs in the cheſt,*(a)* ſome perſons ſuppoſed that it produced exhilarating effects upon the ſyſtem, while others imagined that the employment of this gas was followed by uneaſineſs and pain about the region of the thorax. Theſe different ſenſations muſt be attributed in a great degree to the effects of imagination; in part, however, they may be aſcribed to the gas which was employed in the earlier period of the pneumatic experiments having been often in an impure ſtate, mixed with acid, acrid, or metallic particles.*(b)* A difference in the effects produced by the reſpiration of the gas, ought perhaps in ſome degree alſo, to be imputed to the manner in which it was received into the lungs, whether

only

(a) Obſerv. on Air, v. ii. *p.* 162. *(b)* Note 60.

only in ſmall quantities, or by deep and laborious inſpirations, and whether it was employed in a condenſed or a rarified ſtate.

As Dr. Prieſtley was the firſt perſon who himſelf reſpired oxygenous gas, ſo he was likewiſe the firſt who obſerved the effects which it produced upon animals altogether immerſed in it. His experiments were performed upon mice; they decidedly proved the ſuperior power which this gas poſſeſſes of ſupporting animal life, but no other certain concluſions can be deduced from them.(a) M. Lavoiſier afterwards turned his attention to this ſubject, and in the experiments upon guinea-pigs, to which I have already referred, he noticed with more accuracy the effects reſulting from the reſpiration of oxygene. He examined the ſtate of the internal organs of animals which had been for ſome time confined in this gas, and he conceived that a degree of redneſs and turgeſcence of the veſſels was produced, and other effects which indicated that the ſanguiferous ſyſtem had been in a ſtate of encreaſed action.(b) There is, however, reaſon to infer, that in this caſe, either the gas employed was in an impure ſtate, or that there were ſome circumſtances attending the ſituation

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(a) Note 61.

(b) *Mem. Soc. Roy. Med.* 1782, 1783. *p.* 576.

tion of the animals, or the manner in which the experiment was conducted, which affected the reſults, for the ſame philoſopher, in the ſubſequent memoir of 1789, where there appears to have been the greateſt attention to accuracy, and where the moſt perfect apparatus was employed, forms entirely oppoſite concluſions. In this paper, we are informed, that he confined guinea-pigs in pure oxygene, and in mixtures of oxygene and azote, in different proportions, until the former conſtituted only one-fifteenth part of the compound. In all theſe caſes, he found that the ſame quantity of oxygene was conſumed, a circumſtance, he obſerves, in which reſpiration differs remarkably from combuſtion, though in many reſpects, theſe operations are ſimilar to each other. The effects produced upon the animals were preciſely the ſame, whether they were confined in pure oxygene, or in any of the mixtures of it, except that when the proportion of azote was very large, they exhibited marks of drowſineſs. The author expreſſly informs us, that neither the temperature nor the circulation were in any reſpects affected by the inſpiration of pure oxygene for the ſpace of ſeveral days.(a) Theſe experiments muſt be conſidered as very valuable; there is no reaſon to doubt their accuracy, and they may be relied on with more confidence, as the author ſeems to have had no peculiar theory in view when he performed them; indeed the reſults are different from what we might previouſly have expected, and are unfavourable to the analogy which Lavoiſier had always endeavoured to eſtabliſh between reſpiration and combuſtion.

(a) Note 62.

We have an account given by Dr. Higgins of the reſpiration of pure oxygene by the human ſubject; in one experiment, thirty-eight pints of this gas were reſpired without interruption. No inconvenience was experienced, a ſenſe of warmth was, however, produced in the cheſt, and the pulſe was conſiderably quickened.(a) From the experiments of M. Lavoiſier, I am induced to conjecture, that this encreaſed rapidity of the circulation muſt be attributed rather to the greater efforts which almoſt neceſſarily attend reſpiration when continued for any length of time in the way in which it was performed in this proceſs, than to the effects of the application of the increaſed quantity of oxygene to the lungs. Dr. Higgins, in a 2d experiment, breathed a quantity of oxygene under an additional preſſure, and by very full inſpirations. He conceived that by theſe means its conſumption was much promoted,(a) but more numerous and accurate expeririments will be required before this inference can be fairly eſtabliſhed.

(a) *Minutes of a Society, &c. p.* 144—6.

In the year 1794, Dr. Beddoes publiſhed his experiments upon this ſubject. They were performed upon rabbits, and the attention was principally directed to an examination of the ſtate of the internal organs of the animal, after it had been ſubjected for ſome time to the influence of pure oxygene. The author commences his inquiry, by ſtating, that "Dr. Prieſtley and "M. Lavoiſier, found animals either to die or to "become exceedingly ill in ſuch air," (oxygene little diluted) "while it continues more oxidated "than the atmoſphere."(b) There is no reference made to any particular paſſage in the writings of theſe philoſophers, but from the extracts which I have given, it would appear that their ſentiments are altogether of an oppoſite nature.(c) Dr. Beddoes quotes the earlier experiments of M. Lavoiſier, but the later and more deciſive ones in the memoir of 1789 are not noticed. It is on the contrary taken for granted, that when an animal has been for ſome time confined in air, which contains a greater proportion of oxygene than the atmoſphere, the body becomes, as he expreſſes it, oxygenated. He had previouſly formed an hypotheſis reſpecting the operation of oxygene upon the ſyſtem, according to which the long continued inſpiration of it was ſuppoſed to induce an inflammatory ſtate, and he accordingly found indications of this peculiar condition in every part which he examined. The lungs were florid, and in ſome places they were marked with livid ſpots, the pleura was inflamed, the heart retained its irritability for a longer ſpace of time than uſual, the blood coagulated more rapidly, and the liver was of a lighter colour than ordinary. The whole ſyſtem was, as it were, ſo completely ſaturated with oxygene, that the immerſion in hydrogenous gas, and in water, did not deſtroy the animals, until after a longer interval than uſual; a fact which remarkably coincided with the theory reſpecting the cauſe of death in ſubmerſion, which the author had previouſly formed. The great difference between the reſults of theſe experiments, and thoſe of M. Lavoiſier, will appear the more remarkable when we conſider, that in the experiments of the latter, the animals were expoſed to the influence of the oxygene for ſeveral days, whereas thoſe of Dr. Beddoes occupied a much ſhorter ſpace of time. Indeed we are expreſſly informed, that the air in which the animal had been confined, "ſeemed to "have ſuffered little diminution either in quan-"tity or quality,"(a) notwithſtanding which, theſe ſingular changes were induced upon the body. It is impoſſible to reconcile opinions and experiments ſo contradictory as thoſe which I have related; our judgment muſt in ſuch inſtances be guided partly by the general character for correctneſs and impartiality which we attach to the works of the reſpective authors, and partly by comparing them with the reſults of experiments which in any degree reſemble them.

(a) *Minutes of a Society, &c. p.* 152. (b) *Beddoes on fact. Airs, pt.* i. *p.* 13. (c) See Notes 61 and 62.

We are indebted to Mr. Davy for ſome intereſting experiments upon the reſpiration of oxygene, which appear to have been performed with his accuſtomed correctneſs. He ſuppoſes that life is finally deſtroyed by the long-continued employment of this gas, and refers for the proof of his opinion, not only to the reſults of his own experiments, but to thoſe of Dr. Beddoes and M. Lavoiſier. He does not, however, notice any particular paſſage in the works of this latter author, and conſidering the very direct nature, both of his experiments and opinions upon this point, it may be conjectured, that Mr. Davy has been inadvertently miſled by the aſſertions of Dr. Beddoes. Indeed Mr. Davy's own experiments appear to lead to concluſions diametrically oppoſite to thoſe of Dr. Beddoes. He reſpired the ſame portion of oxygene for ſeveral ſucceſſive times, and then accurately aſcertained the exact quantity of it which had been conſumed, and of carbonic acid which had been generated, when he found, that both theſe quantities were leſs than in the ordinary reſpiration of atmoſpheric air, he obtained the ſame reſults when mice were made the ſubjects of his experiments. (a) It appears therefore, that in this inſtance, if any alteration be produced in the ſyſtem, it muſt ariſe rather from a deficiency than from an exceſs of oxygene. But notwithſtanding the attention with which theſe experiments appear to have been conducted, an objection may, I think, be urged againſt them from the erroneous eſtimate which Mr. Davy formed of the capacity of the thorax in a ſtate of complete expiration, a datum which is employed in the calculation of the quantity of oxygene left after the proceſs. (b)

(a) *Beddoes on Airs, pt.* i. *p.* 16.

From the review of the experiments which have

(a) *Researches, p.* 444.

(b) Note 63.

have been made upon the ſubject, I think myſelf juſtified in concluding, that we have not yet any direct proof that the reſpiration of oxygene is injurious to the animal œconomy. It is, however, not impoſſible that it may be the caſe, were the gas applied to the lungs for a ſufficient length of time, but we have no ground either from experiment or obſervation, to enable us to determine what peculiar morbid action would be induced.

Among the remaining reſpirable gaſes, that which appears to be the leaſt injurious to the living body is the oxide of azote. This aeriform fluid was firſt diſcovered by Dr. Prieſtley, and was by him ſuppoſed to be "in the higheſt degree noxious to animals." *(a)* The ſociety of Dutch chemiſts, who afterwards examined its properties with more accuracy, coincided with Dr. Prieſtley, as to its effects upon animal life. *(b)* The experiment was, however, repeated by Mr. Davy, and he diſcovered not only that this gas may be reſpired for a ſhort time without inconvenience, but that the employment of it is ſucceeded by a ſingular excitement of the nervous ſyſtem, which differs from that produced by alcohol

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(a) Obſerv. on Air, v. ii. *p.* 55. *(b) Journ. de Phys. t.* xliii. *p.* 329.

hol and opium, in not inducing a ſubſequent ſtate of exhauſtion. Mr. Davy has alſo proved, that it is abſorbed by the blood in conſiderable quantity, and he imagines that it is decompoſed by this fluid. *(a)* I have already ventured to expreſs my doubts as to the correctneſs of this latter concluſion. *(b)*

Hydrogenous gas has been frequently reſpired, *(c)* and it is now pretty generally agreed, that it is altogether paſſive when received into the lungs, and that death ſucceeds the employment of it in conſequence of the excluſion of oxygene, in the ſame manner as by ſuffocation, or drowning. M. Lavoiſier diſtinctly aſcertained this fact, in his experiments related in the memoir of 1789, and it has been ſince confirmed by Mr. Davy. *(d)* It muſt be remarked, however, that a contrary opinion reſpecting the effects produced by the reſpiration of hydrogene has been maintained by ſome eminent chemiſts, even by Dr. Prieſtley himſelf; *(e)* but his experiments were made in the earlier period of the pneumatic chemiſtry,

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(a) Researches, p. 409, *& seq.* *(b)* See Note 50.
(c) Scheele on Air and Fire, p. 160. *Fontana, Phil. Trans.* 1779, *& Journal de Phys. t.* xv. *Pilatre de Rozier, Journ. de Phys. t.* xxviii. *p.* 425. *(d) Researches, p.* 466.
(e) Observ. on Air, v. i. *p.* 229.

chemiſtry, when the gaſes were frequently employed in an impure ſtate, and the experiments of Mr. Davy, clearly demonſtrate that hydrogenous gas produces different effects upon the ſyſtem, according to the ſubſtances from which it is procured.(a)

The experiments that have been made upon the ſubject of azotic gas are few and imperfect; it has been generally ſuppoſed, that it exerciſes no noxious effects upon the blood, but like hydrogene, deſtroys life ſimply by preventing the acceſs of oxygene. Dr. Higgins indeed remarks, that an animal dies ſooner when immerſed in this gas, than from the ſimple interruption to reſpiration,(b) but we are not informed upon what data this opinion is founded. Mr. Davy likewiſe experienced the ſenſe of ſuffocation more ſpeedily from the uſe of azote, than from that of hydrogene, but it appears that the gas employed in the experiment contained a quantity of carbonic acid.(c) to which we may, with great probability, aſcribe its noxious effects; and the ſame philoſopher, when ſpeaking in general terms of the action of azote in reſpiration, ſeems to conſider it as merely excluding oxygene.(d) This opinion is adopted by Dr. Thomſon,(a) and was uniformly maintained by M. Lavoiſier.(b) It would certainly appear reaſonable to conclude a priori, that a ſubſtance which enters ſo largely into the compoſition of the atmoſphere, and which conſequently compoſes ſo great a proportion of the contents of the lungs, could not exerciſe any noxious effects upon the animal ſyſtem.

(a) *Researches, loc. cit.* (b) *Minutes of a Society, &c. p.* 133. (c) *Researches, p.* 466. (d) *Do. p.* 334.

The only remaining gas which is capable of being received into the lungs, is the carbonated hydrogene or hydrocarbone. If it be inſpired in an undiluted ſtate, it is followed by inſtant death, and when employed in ſmall quantity only, mixed with atmoſpheric air or with oxygene, if it be uſed for any length of time, it induces vertigo, dimneſs of ſight, convulſions, loſs of ſenſation, and in ſhort, every ſymptom of approaching diſſolution. It evidently acts more rapidly and powerfully than thoſe gaſes which merely exclude oxygene from the blood, and muſt conſequently be conſidered as exerciſing a poſitively noxious influence upon the animal œconomy. Theſe fatal effects have been attributed by ſome authors to its ſuddenly abſtracting all the looſe oxygene from the blood, and thus completely depriving that fluid of the property by which it ſupports the irritability of the muſcular fibres of the heart and arteries. To this hypotheſis it may be objected, that the hydro-carbonous gas does not in other caſes appear to poſſeſs that powerful attraction for oxygene, which could induce ſo rapid a change in the compoſition of the blood; and it may be farther urged, that if it be taken into the lungs, mixed with a quantity of oxygenous gas, provided it be not too much diluted, it will ſtill be found to produce its accuſtomed fatal effects. The noxious influence of this gas is, I conceive, with more propriety referable to its action upon the nervous ſyſtem, upon which it produces *directly sedative* effects. (a)

(a) *Chemistry, v.* iv. *p.* 492. (b) *Mem. Acad.* 1789. *p.* 574. *& alibi.*

All the remaining gaſes are found to be ſtrictly non-reſpirable, i. e. incapable of being admitted into the trachea. It is obvious that this muſt be the caſe with the irritating acid or alcaline gases, and with the nitrous gas, which during its paſſage into the lungs, muſt unavoidably be brought into contact with oxygene, and thus produce nitrous acid vapour The only ſubſtance reſpecting the reſpirability of which there could be any doubt, is the carbonic acid gas, which, though poſſeſſed of the decided characteriſtics of an acid, exhibits them in a much ſlighter degree than that in which they generally exiſt. The impoſſibility of taking it into the lungs, even by the moſt powerful voluntary efforts, when it compoſes a large proportion of the air, was however proved by the experiments of M. Pilatre de Rozier,(a) executed with that intrepidity which formed ſo remarkable a trait in the character of this philoſopher.

(a) Note 64.

With theſe remarks I ſhall conclude the ſecond diviſion of my ſubject, and at the ſame time, the preſent volume. The third part of the eſſay, in which I propoſe to treat of the different affections of reſpiration, muſt principally conſiſt of original obſervations and experiments, and though I have made ſome progreſs in them, they are ſtill in too imperfect a ſtate to meet the public eye. Phyſiologiſts have in general been more inclined to form hypotheſes than to execute experiments, and it has neceſſarily enſued from this unfortunate propenſity, that their ſcience has advanced more ſlowly than almoſt any other department of natural philoſophy. The ſubject of reſpiration has indeed of late attracted the attention of the chemiſts, and its phenomena have been inveſtigated with conſiderable aſſiduity. A proportional advancement in our knowledge has been conſequently produced, but there ſtill remain many queſtions to be ſolved, and many difficulties to be removed. Some of theſe difficulties have appeared in thoſe parts of the ſubject which have paſſed under our review, but they will be found to exiſt in much greater number in the remaining diviſions. How far it will be in my power to remove any of them, it would at preſent be preſumptuous in me to determine, though I think myſelf juſtified in auguring favourably of the events of my experiments, from the intereſting reſults of thoſe which I have already performed.

(a) *Journ. de Phys. t.* xxviii. *p.* 422. *& seq.*

NOTES.

NOTES.

Note I.

M. CUVIER ſtates, that there are ſome very imperfect animals, in which no appearance of reſpiratory organs has yet been diſcovered; (a) but we are induced by analogy to ſuſpect, that in theſe caſes, future and more accurate obſervations will enable us to detect the apparatus by which a function is performed, which ſeems abſolutely eſſential to the exiſtence of animal life.

In conformity with the cuſtom of the modern phyſiologiſts, the word reſpiration is here employed to ſignify that operation by which the blood and the air are enabled to act upon each other, though, according to the etymology of the term, it ought only to be applied to thoſe animals that are furniſhed with lungs. The neceſſity of oxygene for the ſupport of the life of fiſh, was firſt diſcovered by Dr. Prieſtley *(a)*; it has been ſince confirmed by Sig. Carradori *(b)* and others.

(a) *Leçons d'Anatomie Comparée, t.* i. *p.* 42.

Note II.

A good deſcription of the comparative anatomy of the organs of reſpiration may be found in Mr. John Bell's Anatomy, *vol.* ii. *c.* 3.

Note III.

It has been imagined by ſome phyſiologiſts, that inſects differed from other animals in the chemical change which their reſpiration produced in the air, but this opinion is proved to be erroneous by the experiments of Vauquelin. *(c)*

Note IV.

This idea reſpecting the termination of the bronchia,

(a) Experiments on Air, v. iii *p* 382—7. *(b) Ann. de Chimie, t.* xxix. *p* 171, 2. *(c) Ann. de Chim. t.* xii. *p.* 273.

bronchia, which is generally adopted by the modern anatomiſts, reſts altogether, or in a great meaſure, upon the microſcopical obſervations of Malpighi. Succeeding phyſiologiſts have not been able to trace this minute ſtructure, and ſome authors of reſpectability have not ſcrupled to deny its exiſtence. Among others, Helvetius endeavours to prove that the bronchia terminate in a cellular or ſpongy tiſſue, compoſed of a delicate or membranous ſubſtance, the cells of which have no determinate figure, or regular connexion with each other. *(a)* There is indeed every reaſon to ſuppoſe with Cheſelden, *(b)* that Willis and other ſubſequent anatomiſts, who have deſcribed and delineated diſtinct, rounded veſicles, have exceeded the limits of accurate obſervation. Malpighi, upon whoſe authority the belief of the exiſtence of this peculiar ſtructure is founded, expreſſes himſelf in the following manner, "Diligenti indagine inveni totam pulmonum molem, "quæ vaſis excurrentibus appenditur, eſſe aggregatum quid ex leviſſimis & tenuiſſimis membranis, quæ extenſæ & ſinuatæ pene infinitas "veſiculas orbiculares, & ſinuoſas efformant, veluti in apum favis alveolis ab extenſa cera in "parietes conſpicimus." And again, "membranæ iſtæ veſiculæ videntur efformari ex de- "ſinentia

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(a) Mem. Acad. 1718. *(b) Cheselden's Anatomy, p.* 178.

“ ſinentia tracheæ, quæ extremitate, et lateribus “ in ampulloſos ſinus faceſſens, ab his in ſpatia, “ & veſiculas inæquales terminatur.”*(a)* This deſcription by no means conveys the idea of the regularly rounded veſicle, which has been adopted by ſucceeding anatomiſts, and is delineated by Willis.*(b)* The analogy of the frog and tortoiſe is certainly in favour of the opinion of Helvetius; the ſtructure of their lungs, which is leſs minute, evidently diſcovers the termination of the bronchia to be a congeries of irregular, membranous cavities. Haller, with his accuſtomed candour, after ſtating the oppoſite authorities, confeſſes that he was at one time inclined to doubt the generally received opinion, although he afterwards ſaw reaſon to acquieſce in the doctrine of Malpighi.*(c)* M. Dumas, who gives a full account of the different opinions that have been entertained upon this ſubject, ſeems inclined to adopt an intermediate one between that of Helvetius and Haller.*(d)*

Note V.

Many phyſiologiſts, ſanctioned by the authority

(a) Epist. de pulm. i. *(b) Pharm. Rat. sect.* i. *cap.* i.; *Tab.* 3. *(c) El. Phys.* viii. 2. 29, 30. *& Notæ ad Boer. Prælect. t.* ii. *p.* 154, 5. *(d) El. Phys. t.* iii. *p.* 435—9.

rity of Galen and Harvey, have ſuppoſed, that there was a quantity of air contained between the lungs and the thorax; Hoadley *(a)* and Hales *(b)* endeavoured to prove this point by experiment. Indeed ſo prevalent was this opinion, even half a century ago, that Haller thought it neceſſary to apologize for embracing the oppoſite ſide of the queſtion; his philoſophic language on the occaſion deſerves to be quoted. "Candide, "nulla parte ſui roboris diſſimulata, adverſariorum "argumenta propoſui; ſuperat, ut oſtendam, "cur ab iis aut auctoritatibus, aut experimentis "non paſſus ſim me perſuaderi. Neque enim "hic quid-quam valuit aut præceptoris mei gratia, aut adfinis mei, adque eam uſque primam "illam Boerhaavianæ ſententiæ propugnationem "ſatis amici, Hamburgeri odium, neque concertationis inde natæ tædium; neque ulla certe "cauſa mei diſſentiendi eſt, præter ea, quæ "nunc produco, rationum momenta. Neque me "malus pudor retinuerit. Facile enim video ſiquidem contrariam meæ opinionem expenſam "præponderare ſentirem, veram me gratiam, "ſinceramque laudem, apud candidos virtutis "æſtimatores meriturum, ſi ex longo opere, "quod in ætatem durare velim, errorem etiam

"ſero

(a) Lectures on Respiration, chap. i. *(b) Statical Essays, v.* ii. *p.* 81.

" ſero adgnitum proſcriberem." *(a)* Many anatomiſts and phyſiologiſts of the firſt reſpectability conceived that they had detected ſmall pores in the veſicles of the lungs, which permitted a part of the air in them to tranſude into the cavity of the thorax, between the pleuræ ; *(b)* but later obſervations have decided againſt the exiſtence of theſe paſſages.

Note VI.

The opinions of the moſt learned amongſt the ancients reſpecting the nature and uſe of the diaphragm, are very remote from the ideas which we now entertain upon this ſubject. Fabricius ſeems to have been the firſt who clearly pointed out its muſcularity, and its great importance in the act of reſpiration. *(c)* Some anatomiſts, among others Senac *(d)* and Winſlow, *(e)* from the connexion of the centre of the diaphragm with the parietes of the thorax and the mediaſtinum, were led to ſuppoſe, that this part remained immovable, and that it was the ſides of it only which had their poſition changed by the contraction

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(a) Haller, viii. 2. 5. *Dumas, t.* iii. *p.* 423. *& seq.* *(b) Boerhaave Prælect. t.* v. *pars.* i. *p.* 22. *(c) Fabricii Op. De Resp. c.* 8. *&c.* *(d) Mem. Acad.* 1724, *&* 1729. *(e) Mem. Acad* 1738.

tion of its muſcular fibres; but Haller, and moſt of the modern anatomiſts conceive that the whole of the diaphragm is lowered during inſpiration, *(a)* although it is generally admitted, that the degree of motion in the centre is leſs conſiderable.

Note VII.

The conteſts of anatomiſts on this ſubject have been at leaſt as violent, as reſpecting the exiſtence of air in the cavity of the thorax; *(b)* Haller prefaces his account of the controverſy with the following remark: "Multo plus de "iſtorum muſculorum officio dubitationis ab "omni tempore fuit, cumque acres inimicitias "harum carnium actio mihi conciverit, accura"tius erit in hac quæſtione verſandum: at in"terim liceat, inter miſerias vitæ humanæ etiam "iſtam deploraſſe, potuiſſe ob res adeo ab omni "utilitate propria, atque conſervatione noſtri "remotas, tantas iras, tantaſque & tam acer"bas nobis intentare lites." *(c)* The older anatomiſts generally ſuppoſed from the ſituation and ſtructure of the intercoſtals, that the externals by their contraction increaſe the capacity of the cheſt, and conſequently ſerve for inſpiration, but that the internals on the contrary diminiſh the

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(a) Haller, viii. 1. 36. *(b) Dumas, t.* iii. *p.* 492—6. *(c) Haller*, viii. 1. 13.

cavity of the thorax. This opinion has been adopted by ſome of the more modern phyſiologiſts; *(a)* ſo late as the year 1778, Sabatier, both from obſervations made upon human ſubjects who had received wounds in the thorax, and from experiments performed for the expreſs purpoſe upon dogs, concludes that the action of the intercoſtals, both external and internal, tends to contract the cheſt, and that they therefore belong to the muſcles of expiration. *(b)* The contrary opinion, that both ſets of intercoſtals ſerve for inſpiration, was firſt ſtarted by Mayow; *(c)* Haller ſtrenuouſly defended it, and at preſent this opinion is generally received. He forms a comparative eſtimate of the increaſe of capacity which the thorax receives from the action of the diaphragm and of the intercoſtals. *(d)* The ſame author remarks, after Winſlow and Senac, that the action of the intercoſtals is principally uſeful in thoſe caſes where, from any accidental cauſe, the contraction of the diaphragm is impeded.

From an accurate inſpection of the ſkeleton, and from obſervations made upon the living body,

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(a) Hoadley's Lectures, p. 8. *(b) Mem. Acad.* 1778. *Richerand, El. phys. p.* 138. *(c) Tract. quinque, p.* 278, *& seq.* *(d) Boer. Prælect. t.* v. *pars* i. *p.* 84—9; *El. phys.* viii. 4. 6.

body, anatomiſts are, for the moſt part, agreed in ſuppoſing, that there is ſome difference in the manner in which the function of reſpiration is carried on in the male and female ſex; the former appearing to make more uſe of the diaphragm, and the latter of the muſcles about the thorax. *(a)* It is ſcarcely neceſſary to remark, that by this variation of ſtructure, the impediments to reſpiration, which would otherwiſe have been produced by a ſtate of pregnancy, are in a conſiderable degree obviated.

Winſlow ſeems to have committed a miſtake in ſpeaking of the opinion of Fabricius, reſpecting the intercoſtals; he ſays, "Ab aquapendente "(Fabricius) avoit dés l'an 1599, trés bien de"montré, que les muſcles internes & externes "concourent enſemble à la même action, c'eſt-a"dire, a lever les cotes." *(b)* Fabricius himſelf employs the following words: "Quapropter dica"mus nos muſculos externos intercoſtales coſta"rum dilatare, internos vero contrahere, pluri"bus adducti tum auctoritatibus, tum rationi"bus." *(c)* And again, "Concludendun igitur "eſt externos intercoſtales thoracem dilatare, in"ternos vero contrahere." *(d)* Haller has alſo

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(a) Boer. Prælect. t. v. *p.* 145. *(b) Mem. Acad.* 1738.
(c) Fabricii, Op. p. 176. *(d) Ditto, p.* 177.

fallen into the ſame inaccuracy with reſpect to this author. *(a)*

Note VIII.

The celebrated Boyle appears to have been the firſt, who pointed out the real cauſe of the influx of the air into the lungs, when the capacity of the thorax is increaſed. *(b)* Some good obſervations were alſo made upon this ſubject by his contemporaries, Sylvius, *(c)* Borelli, *(d)* and Mayow. *(e)* Succeeding phyſiologiſts for the moſt part acquieſced in the opinion of Boyle, though ſome of them, not contented with his ſimple explanation, have perplexed by their ſubtle hypotheſes, a ſubject in itſelf ſufficiently intelligible. *(f)* The proceſs is briefly, yet clearly, detailed by Haller in the following ſentence. "Si "pulmonem, & una pectus, dilataveris, ut aer "pulmonalium veſicularum in amplius ſpatium "diffuſus rareſcat, pro portione, qua is aer at-"moſphæra incumbente rarior fit, pro ea co-"lumna aerea in eum pulmonem deſcendet, eo

"celerius,

(a) El. phys. viii. 1. 14. *(b) Boyle's Works, v.* i. *p.* 99. *&* *seq.* *(c) Franc. de le Boe Sylvii Opera, p.* 16. *(d) De Motu Anim. p.* 2. *prop.* 82, 83. *(e) Tract: quinque, p.* 271. *&* *seq.* *(f) Baglivi Op. p.* 454. *Hoadley's Lectures, p* 12. *Bremond, Mem. Acad.* 1739.

" celerius, quo differentia denſitatum major
" erit."

The firſt operation therefore is the expanſion of the thorax, which is effected by muſcular contraction; as there is no air between the pleuræ, the lungs act as if ſuſpended in a vacuum; they conſequently follow the expanſion of the thorax, and the external air ruſhes into the veſicles, as into the partially exhauſted receiver of an air pump, when a communication is formed with the atmoſphere. The air enters the lungs becauſe they are expanded; the lungs are not, as was formerly imagined, puſhed open by the force of the air ruſhing into them. *(a)*

Note IX.

Many of the older anatomiſts ſuppoſed, that the lungs were endowed with a certain innate power of motion, by which the alternations of expanſion and contraction might be carried on, independently of the change in the capacity of the cavity containing them. They even attempted to eſtabliſh this poſition by experiment; but in theſe inſtances there muſt have been ſome fallacy.

(a) Boer. præl. t. v. pars i. *p.* 15. 49, 50.

fallacy. We know of no method in which animal motion can be produced, except by the contraction of the muſcular fibre, and though a muſcular ſtructure in the veſicles has been confidently aſſerted by ſome writers of eminence,(*a*) later and more accurate obſervations have decided againſt its exiſtence. Helvetius has even endeavoured to prove, that the fibres which are obſerved between the rings of the trachea, and the larger branches of the bronchia, are not muſcular,(*b*) but their muſcularity is admitted by Winſlow, Haller, and moſt other eminent anatomiſts. (*c*) The lungs, in common with other bodies compoſed of cellular texture, poſſeſs a degree of elaſticity, by means of which, when ſtretched beyond a certain limit, they will re-act and expel part of their contents. In the proceſs of reſpiration, however, they are entirely paſſive, as they always remain in complete contact with the parietes of the thorax.

Notwithſtanding theſe conſiderations, which have very generally induced the modern phyſiologiſts to diſcard the idea of motion, originating in

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(*a*) *Willis, pharm. Rat. p.* 9. *Malpighi, Phil. Trans.* 1671. *Bremond, Mem. Acad.* 1739. (*b*) *Mem. Acad.* 1718. (*c*) *Winslow's Anat. Sect.* 9. §. 133. *Haller, Phys.* viii. 2. 12.

in the lungs themſelves, it appears, from the following paſſages in the Zoonomia, that Dr. Darwin ſtill adheres to the old opinion, as he directly attributes their action to a cauſe independent of the contraction of the diaphragm or muſcles of the thorax. "By the ſtimulus of the blood in "the right chamber of the heart, the lungs are "induced to expand themſelves, and the pecto-"ral and intercoſtal muſcles, and the diaphragm, "act at the ſame time by their aſſociations with "them." *(a)* And again, "to theſe increaſed "actions of the air cells, are ſuperadded thoſe of "the intercoſtal muſcles and diaphragm, by irri-"tative aſſociation." *(b)*

M. Dumas alſo adopts the ſame opinion, and conjectures, that the lungs are poſſeſſed of an innate power of motion; he even expreſſes his concurrence in the hypotheſis of M. Bremond, who inferred from one of his experiments, that the contraction and dilatation of the lungs alternated with that of the thorax, and conſequently muſt depend upon ſome other cauſe. *(c)*

When the lungs are removed from the body, and

(a) Zoonomia, v. i. *p.* 40. *(b) Ditto,* v. ii. *p.* 50.
(c) El. phyſ. t. iii. 440, 1.

and the paſſage into the trachea remains open, they gradually become collapſed, and are contracted into a ſmaller ſpace than they occupied whilſt in the cavity of the thorax. This has been generally aſcribed to the re-action of their elaſtic cartilages and membranes, which, in conſequence of the preſſure of the internal air, not being balanced by any air between the pleuræ, had been, while in the body, retained in a ſtate of over-diſtention. Boerhaave and his commentator Haller, attribute part at leaſt of this effect to the contraction of the muſcular fibres of the trachea and bronchia; *(a)* but it may be obſerved in oppoſition to ſuch great authorities, that the contractile power of the lungs, remains for a long time after their removal from the body, and cannot therefore with propriety, be aſcribed to a cauſe which muſt ceaſe with the vitality of the part.

Note X.

An elaborate account of theſe muſcles, and of the effects produced upon the thorax by their contraction, will be found in *Boer. prælect. t.* v. *pars* i. *p.* 126—144, *and Haller* viii. 1. 17—31.

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(a) Boer. prælect. t. v. *pars* i. *p.* 6.

The moſt eminent anatomiſts have differed in opinion, reſpecting the degree of motion which the ribs exerciſe in reſpiration; while ſome authors, with Borelli *(a)* have conceived the action of the intercoſtals to be eſſential, others have adduced inſtances in which the cartilages of the ribs were completely oſſified, without any conſequent impediment to this function. *(b)* It is alſo acknowledged, that in the moſt accurate obſervations and experiments upon the living body, the motion of the ribs was ſcarcely viſible to the eye. Upon the whole, it ſeems the moſt probable, that they are nearly at reſt in ordinary reſpiration, but that in the moſt violent actions of the thorax, or in caſes where the contraction of the diaphragm is impeded by accident or diſeaſe, they are employed to compenſate for the deficiencies of this organ. But though there has been ſome difference of opinion reſpecting the degree of the motion of the ribs, and the occaſions on which

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(a) Borelli, p. ii. *prop.* 84.

(b) Winslow Mem. Acad. 1738; *Fabricius de Resp. c.* x. *sub finem*; Haller quotes *Cheselden, osteog. c.* iii. as recording ſome caſes where no injury was produced to the reſpiration by anchyloſes of the ribs; but this appears to be an overſight; Cheſelden ſays, "I have twice found them" (the cartilages which join the ribs and ſternum) "totally oſſified in men between 40 and "50 years of age, both of which died with a great diffi-"culty of breathing."

which it is exerted, it is unanimouſly acknowledged, that the mechaniſm of their articulations is peculiarly admirable. According to the obſervation of Bellini, the thorax is increaſed in all its dimenſions, while its figure remains unchanged. (*a*)

The authors who have treated this ſubject with the moſt accuracy are, *Borelli, p.* ii. *prop.* 81—95. *Senac, Mem. Acad.* 1724. *Winslow, ibid.* 1720 & 1738. *Boer. Instit.* §. 615 ; *prælect. passim. Haller, phys. lib.* viii. *passim. Dumas, El. phys. t.* iii. *p.* 429—434.

Note XI.

An account of the various eſtimates which were formed by the older phyſiologiſts may be found in *Haller, El. phys.* viii. 4. 6.

Note XII.

Theſe eſtimates were formed from perſons who had died of diſeaſe, the capacity of the lungs after hanging he found to be conſiderably greater ; the average taken from experiments made

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(*a*) *Lemm.* 11.

made upon three bodies, was 260 cubic inches. The author, however, obſerves, that fear always produces a deep inſpiration, and that on this account, in conſequence of the peculiar circumſtances of their death, their lungs would be in a ſtate of extraordinary diſtention.(*a*) Mr. Coleman, on the contrary, from an experiment made for the expreſs purpoſe upon a dog, concludes, that after hanging, the lungs contain only a very ſmall quantity of air.(*b*) During the violent ſtruggles which precede death, he ſuppoſes that the animal retains the power of expelling air from the trachea, though the preſſure of the cord prevents any from being received. My friend Dr. Skey, in order to obviate the apparent contradiction between Dr. Goodwyn's and Mr. Coleman's experiments, ſuggeſted that the lungs of a man after hanging muſt contain more than their uſual quantity of air, becauſe from the effects of terror, a full inſpiration would be made immediately before death, and the air thus received muſt be, for the moſt part, retained by the preſſure of the cord, while in an animal unconſcious of its approaching fate, it will be a matter of accident whether the lungs were in a ſtate of inſpiration or expiration at the moment of death. But I think that

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(*a*) *Goodwyn, p.* 25, 26. (*b*) *p.* 97, 8.

that this idea, though ingenious, is ſcarcely ſufficient to remove the difficulty. The capacity of the lungs in the three criminals examined by Dr. Goodwyn, coincides very nearly with the eſtimate of the ordinary capacity of the thorax, deduced from another method of calculation. We muſt therefore either ſuppoſe, that in hanging, the compreſſion of the trachea is produced ſo ſuddenly as to retain the thorax in its ordinary ſtate of diſtention, or if we conceive that an additional quantity of air is received from the effects of fear, we muſt conclude that it is expelled at the moment when the ſenſe of ſuffocation firſt begins to be experienced, but that the paſſage being almoſt inſtantly cloſed, prevents the farther evacuation of the lungs. Upon the whole, I am inclined to the latter ſuppoſition.

With reſpect to Mr. Coleman's opinion, he informs us that "this experiment was often re-"peated, and ſometimes ſcarcely any air could be "expreſſed from the lungs." "In all inſtances, "the quantity of air that remained was very in-"conſiderable." The uniformity of the reſults ſeems ſcarcely compatible with Dr. Skey's ſuppoſition. Mr. Coleman ſtates the following propoſitions among the general concluſions which he deduces from his experiments.

" 26.

" 26. That where reſpiration is ſuſpended, " from ordinary hanging, the animal has the " power of expelling air from the lungs.

" 27. That although the muſcles of expiration perform their office, no power can be ap- " plied to open the trachea to admit air.

" 28. That as no air can be received, the " animal dies with the ſame collapſe of the lungs " from hanging as from drowning."

In ſpeaking of the capacity of the thorax in its different ſtates of diſtention, Mr Davy remarks that, " Dr. Goodwyn, in his excellent work on " the connexion of life with reſpiration, has de- " tailed ſome experiments on the capacity of the " lungs after *natural expiration*. He makes the " medium capacity of the lungs about 109 cubic " inches, which agrees very well with my eſtima- " tion."(a) But this ſtatement of Dr. Goodwyn's opinion is not correct; in the part to which Mr. Davy refers, Dr. Goodwyn ſays, " we ſhall " for the preſent adopt the medium quantity of " theſe latter experiments, and ſay that the lungs " of the human ſubject contain 109 cubic inches " of air after *complete expiration*."(b)

(a) *Researches*, p. 411. (b) *Goodwyn*, p. 27.

Note XIII.

I have fixed the quantity of air which can be expelled from the lungs, after an ordinary expiration, at 170 cubic inches, becauſe this is nearly the average of the eſtimates which have been given by different authors, and alſo becauſe I find, that it coincides with ſome trials which I have myſelf made upon the lungs of different individuals. The opinion of Mr. Kite on this ſubject, ſeem to be evidently incorrect. "I "find," he ſays, "by repeated experiments, "that a perſon in health, and in a ſtate of perfect reſt, uſually reſpires about 17 cubic inches "of air; but, at the end of the expiration, there "ſtill continues in the lungs 87 cubic inches," &c. *(a)* From the above quotation it appears, that he either entirely overlooked the quantity which we are now inveſtigating, or confounded it with the air remaining after a complete expiration.

Note XIV.

Mr. Davy indeed remarks, that "as moſt "gaſes, though of different ſpecific gravities "when brought into contact with each other, aſſume

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(a) Essays, p. 47.

" aſſume ſome ſort of union, it is more than " probable, that gas inſpired into the lungs, from " being placed in contact with the reſidual gas " on ſo extenſive a ſurface, muſt inſtantly min- " gle with it." *(a)* But in oppoſition to this argument, we have the direct experiments of M. Jurine, who found the carbonic acid to exiſt in very different proportions in the different parts of the bronchial veſſels. *(b)* Yet the gaſes naturally contained in the lungs, poſſeſs many circumſtances favourable to their equable diffuſion, which would not operate in the caſe of the hydrogene reſpired by Mr. Davy. The perpetual motion which the lungs experience, the gradual production of the carbonic acid, the length of time during which it remains in contact with the other gaſes, would all tend to promote its uniform mixture with them. It may likewiſe be remarked, that carbonic acid, though conſiderably heavier than the conſtituents of the atmoſphere, differs leſs from them in ſpecific gravity than the hydrogenous gas. From the experiments of Fontana, we learn alſo, that hydrogenous gas, even when mixed in a glaſs jar with oxygene or common air, does not diffuſe itſelf equally through the veſſel, but remains in a greater

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(a) Researches, p. 506—7. *(b) Enc. Meth. Medecine t.* i. *p.* 494.

greater proportion in the upper part of it. *(a)* Dr. Higgins informs us, that when an animal is confined for ſome time in a jar of air, the carbonic acid is found to exiſt in greater quantity in the lower than in the upper part of the veſſel. *(b)* In the two experiments on the reſpiration of hydrogene, related by Mr. Davy, there is a difference in the reſults of about $\frac{1}{18}$ part; *(c)* when the larger quantity of hydrogene was employed, the capacity of the lungs appeared greater, becauſe a larger quantity of the gas being forced into the veſicles, a greater proportion of the reſidual air was neceſſarily expelled.

With reſpect to Mr. Davy's eſtimate of a ſingle inſpiration, although in the experiments made with the mercurial apparatus, he fixes it at 13 inches, in another part of the eſſay, he informs us, that his lungs in a ſtate of natural inſpiration, contain 135 inches, and of natural expiration 118 inches; but the difference between theſe quantities, which will meaſure the bulk of a ſingle inſpiration, is 17 cubic inches. *(d)*

Note XV.

Borelli remarks, that both from the ſhape and texture

(a) Phil. Trans. 1779. *p.* 358. *(b) Minutes of a Society, &c. p.* 136. *(c) Researches, p.* 402—6. *(d) Researches, p.* 410.

texture of the bronchia, and the manner in which the diaphragm acts, it is evident that the lungs cannot expel nearly all the air which they contained.(*a*) Haller conceives it impoſſible that the lungs can be contracted even to one half of their natural dimenſions, unleſs when removed from the thorax, and deprived of air by the operation of boiling.(*b*)

Note XVI.

The tendency which water has to aſſume the aeriform ſtate, when it is freed from external preſſure, is well illuſtrated by M. Gay-Luſſac (*c*) and by Mr. Dalton,(*d*) in their reſpective eſſays upon the dilatation of the gaſſes. From the experiments which were performed by many of the older phyſiologiſts upon different animal ſubſtances placed in the vacuum of the air-pump, we learn, that the blood and the mucous ſecretions poſſeſs the power of extricating gas in a much greater degree than water.

Note XVII.

This method of explaining the reſults of Mr.

(*a*) *De Motu Anim. p.* 2. *prop.* 94. (*b*) *Phys.* viii. 4. 11. (*c*) *Ann. de Chim t.* xliii. (*d*) *Manchester Mem. v.* v.

Coleman's experiments was, I believe, firſt pointed out by Mr. Allen in his lectures.

Note XVIII.

In eſtimating the bulk of a ſingle inſpiration at 40 cubic inches, I have been principally influenced by the experiments of Dr. Menzies, as they appear to be the moſt accurate in their execution, the leaſt exceptionable with reſpect to the nature of the proceſſes employed, and the moſt numerous and varied of any that have been performed upon this ſubject. In favor of this opinion we have alſo the authority of Jurine, Sauvages, *(a)* Hales,*(b)* Haller,*(c)* Blumenbach, *(d)* Chaptal, *(e)* and Bell;*(f)* Richerand, *(g)* alſo eſtimates the bulk of a ſingle inſpiration at between 30 and 40 cubic inches, and Fontana *(h)* at 35.

Note XIX.

There is ſcarcely any opinion in phyſiology, however abſurd it may at firſt view appear, which has not found ſome ſupporters; and accordingly it

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(a) Haller, viii. 4. 6. *(b) Statical Essays, v.* i. *p.* 238. *(c) El. Phys.* viii. 4. 6. *(d) Instit. Phys. p.* 113. *(e) Chemistry, v.* i. *p.* 133. *(f) Anatomy. v.* ii. *p.* 193. *(g) Phys. p.* 147. *(h) Phil. Trans.* 1779. *p.* 349.

it appears that arguments have been urged to prove that the fœtus breathes whilſt in the uterus. *(a)*

Note XX.

This is the hypotheſis which is advanced by Haller in the *El. Phys.* viii. 5, 2; in his notes upon Boerhaave, which were publiſhed at an earlier period, he ſuppoſes that the firſt reſpiration is produced by the cry of the animal, in conſequence of the unpleaſant feelings which it experiences immediately after birth, from cold, preſſure, &c. *(b)* This, however, cannot be conſidered as any explanation of the phenomenon, as it is evident, that there muſt have been a previous inſpiration before the cry could have been produced.

Note XXI.

I conſider an inſtinctive action to be one which is attended with ſenſation, and directed to ſome uſeful purpoſe, but which has been derived neither from experience, obſervation, nor tradition.

Note XXII.

Though it is undeſirable to occupy the attention

(a) *Haller*, xxix. 4. 54; *Whytt on Vital Motions, sect.* 9. *Boyle's Works, v.* i. *p.* 110. *(b) Prælect. t.* v. *p.* ii. *p.* 456.

with exploded theories, yet as a part of the hiſtory of ſcience, it is in ſome degree neceſſary, and in many caſes highly intereſting, to take a curſory view of the doctrines that have been ſupported by men of celebrity, and which for a time have enjoyed a ſhare of popularity. For this reaſon I ſhall ſubjoin a ſhort ſketch of ſeveral different hypotheſes, that have been formed to account for the firſt inſpiration, and for the ſubſequent alternation of inſpiration and expiration, beſides thoſe, which, in conſideration of their own ſuperior merit, or the more diſtinguiſhed celebrity of their reſpective authors, I have thought proper to admit into the body of the work.

1. With reſpect to the cauſe of the commencement of reſpiration;

Borelli imagined that the blood in its paſſage through the lungs, received from the air certain particles, which by communicating to it a vibratory or oſcillatory motion, adapted it for the purpoſes of life, and rendered it proper for ſupporting the circulation. The blood of the fœtus, he ſuppoſed, acquires theſe particles from the blood of the mother, but that as ſoon as the communication is deſtroyed between them, it becomes neceſſary for the ſame action to be performed by the lungs of the young animal itſelf. *(a)*

Pitcairne conceived it to be a general law of the animal œconomy, that a muſcle which is without an antagoniſt, muſt perpetually undergo an alternation of relaxation and contraction, and as the force of the muſcles of inſpiration ſo far exceeds that of the muſcles of expiration, he conſiders the diaphragm and intercoſtals as without antagoniſts, and applying his general hypotheſis to the peculiar caſe of the action of the thorax, he ſuppoſes that the alternate action of the diaphragm muſt commence immediately after birth. *(b)*

Petit conjectures, that the animal ſpirits are, from the firſt exiſtence of the fœtus, flowing downwards into the muſcles of reſpiration, and tending to produce their contraction; but that in conſequence of the preſſure which the animal experiences while in the uterus, theſe muſcles are not at liberty to act. As ſoon however, as the poſition of the new-born animal permits this effect to be produced, the thorax becomes enlarged from the contraction of the diaphragm and intercoſtals,

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(a) De Motu Anim. pars ii. *prop.* 118. *(b) Diſsert. p.* 62.

coſtals, and the air conſequently ruſhes into the veſicles. (a)

Boerhaave imagines it a ſufficient ſolution of this difficulty to ſtate, that the fœtus in the time of birth, in conſequence of its ſtruggles, puts all its muſcles into action, and among others the diaphragm. From this cauſe the thorax becomes expanded, and the air ruſhes into it; the blood then flows through the veſſels of the lungs, and reſpiration ever after proceeds as in the adult animal. (b)

The method employed by Dr. Hartley to account for the commencement of reſpiration, bears a conſiderable reſemblance to the hypotheſis of Boerhaave. He ſuppoſes that reſpiration and crying are excited in the new-born child from the cold, handling of the midwife, and other vivid ſenſations, impreſſed immediately upon its coming into the world. Theſe ſenſations tend to throw the whole ſyſtem of muſcles into action, and in thoſe caſes, where there are antagoniſts, the ſtronger will overcome the weaker, and contraction will be conſequently produced in them. (c)

(a) *Mem. Acad.* 1733. (b) *Instit.* §. 691. (c) *Observations on Man, v.* i. *p.* 95.

According to M. Buffon, when the animal leaves the uterus, the air acts upon all the organs of ſenſation, but particularly upon the olfactory nerves. They are by this means ſtimulated, and the animal makes an attempt to ſneeze; the cavity of the cheſt is thus enlarged, and the air has liberty to flow into the lungs, and the veſicles become dilated. But the air which is received into them has its temperature increaſed, and being expanded, cauſes the re-action of the fibres of the veſicles, which forces it again out of the cheſt. *(a)*

Prof. Blumenbach ſuppoſes, that upon the ſtoppage of the circulation between the mother and the fœtus, a ſenſe of ſuffocation is experienced by the latter. This circumſtance, together with the external cold, and the other new ſtimuli to which the body is expoſed, cauſes a variety of motions to take place, and among others the dilatation of the cheſt, and thus produces the firſt inſpiration. *(b)*

2. With reſpect to the immediate cauſe of the ſucceſſive alternations of inſpiration and expiration;

Some

(a) Nat. Hist. t. ii. *p.* 446. *(b) Instit. Phys. p.* 117

Some of the attempts that have been made to reſolve this problem, as thoſe of Willis and Pitcairne, have proceeded upon the aſſumption of principles, altogether inconſiſtent with the actual laws of the animal œconomy. The former of theſe authors conceived, that during muſcular contraction, the animal ſpirits are conveyed from the tendon to the fibres of the muſcle itſelf, and in the ſubſequent relaxation return into the tendon. In the animal functions this operation is ſuppoſed to be under the power of the will, but in the muſcles ſubſervient to the natural functions, the changes are effected independently of the will, in regular alternation. He attributes the difference in the operation of theſe parts, to the origin whence they derive their nerves, which in the former caſe proceed from the cerebrum, and in the latter from the cerebellum. *(a)*

Pitcairne imagined that all muſcles which are without antagoniſts, are neceſſarily in a perpetual ſtate of alternate contraction and relaxation, an effect which he aſcribes to the interrupted influx of the animal ſpirits into theſe muſcles, in conſequence of the alternate compreſſion which the brain experiences from the dilatation of the arteries. *(a)*

The

(a) Pharm. Rat. p. 18.

(b) Dissert. p. 62.

The learned mathematicians Borelli and Bellini, ſimply ſtate the neceſſity of this regular action to the exiſtence of the animal; and appear to reſt ſatisfied with pointing out its final cauſe. (*a*)

Other phyſiologiſts have endeavoured to account for the phenomenon, by ſome circumſtance in the ſtructure, or mechanical action of the thorax, or of the parts contained in it. Dr. Martine explains the action of the organs of reſpiration, by ſuppoſing, that the lungs, when in a ſtate of diſtention, compreſs the phrenic nerve, and thus produce a relaxation of the diaphragm and intercoſtals, to which theſe nerves communicate ſenſibility. (*b*)

A peculiar theory was adopted by Boerhaave to explain this phenomenon; he imagined that in the ſtate of full inſpiration, the lungs were ſo much diſtended as not to permit the blood to be tranſmitted through them with its accuſtomed facility. Leſs blood will therefore be received by the left ventricle, and conſequently leſs ſent to the cerebellum, and to the nerves which are diſtributed to the organs of reſpiration; hence ariſes a deficiency

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(*a*) *Borelli, p.* ii. *prop.* 117. *Bellini, Lem.* 18. (*b*) *Edin. Med. Essays, v.* i.

a deficiency of the action of thoſe parts, and a conſequent relaxation of the muſcles which ſerve to increaſe the ſize of the thorax. The diſtended ſtate of the thorax being removed, the blood has liberty to flow freely into the left ventricle, and is conſequently ſent in its proper proportion to the cerebellum, it therefore excites the muſcles of inſpiration to contract, and brings back the lungs to their former condition. *(a)*

Dr. Hartley applied his doctrine of vibrations to the ſolution of this problem. He conceives that his peculiar theory explains in an eaſy manner, the propenſity to alternate contraction and relaxation, which is obſerved in many of the muſcles, and upon this general principle, he endeavours to explain the action of the diaphragm; he however attributes the effect in part to aſſociation or habit, and in part alſo to impreſſions made upon the pleura and peritonæum, exciting vibrations in them which are communicated to the diaphragm and muſcles of the abdomen. *(b)*

I have ſtated the opinion of Profeſſor Blumenbach, reſpecting the cauſe of the firſt inſpiration;

(a) *Instit.* §. 619, 620. (b) *Observ. on Man*, *v.* i. *p.* 96.

ration; in conſequence of this action, the lungs become dilated, and the blood flows through them. The air and the blood then act upon each other, and the former of theſe ſubſtances undergoes that peculiar change, which renders it unfit for the farther ſupport of life. It is then diſcharged from the lungs, in conſequence of the efforts of the vis medicatrix naturæ, "naturæ "medicatricis conaminibus," and makes room for the influx of a new portion of air. *(a)*

Dr. Darwin has not formally propoſed any hypotheſis, to account for the alternations of reſpiration, but it may be inferred from ſome expreſſions in the Zoonomia, that he conſidered the actions of inſpiration and expiration to ſucceed each other in conſequence of aſſociation. To this explanation of the phenomenon, it may be objected, that according to the definition of the author himſelf, a complicated ſeries of actions can only become aſſociated together "*by habit*; "*i. e. by frequent repetition.*" *(a)* But the young animal, from the firſt inſtant of its birth, poſſeſſes as complete a power over the muſcles of reſpiration, as it ever acquires during the courſe of its exiſtence. Thoſe caſes, in which for a few

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(a) Instit. phys. p. 117. *(b) Zoonomia, v.* i. *p.* 13.

few moments, a difficulty has occurred in the commencement of reſpiration, are to be referred to a mechanical obſtruction in the trachea, preventing the acceſs of the air, or to ſome other accidental circumſtance, for they are by no means of frequent occurrence, and they are in general immediately relieved by a change of poſture, or by the removal of mucus from the fauces. The actions which are acknowledged to be compoſed of a number of aſſociated motions, ſuch as dancing, or playing upon a muſical inſtrument, though many of them leſs complicated than the act of reſpiration, can only be acquired by long practice, and by repeated efforts of attention.

Note XXIII.

The celebrated Harvey, ſeems to have been the firſt who ſtated, that the action of the heart and lungs are totally independent of each other. He however imagines, that the paſſage of the blood along the pulmonary veſſels, is aſſiſted by the motion of the thorax. *(a)*

The connexion which was ſuppoſed by ſome of the older writers, to exiſt between the motions of

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(a) De Motu Cord. p. 11. 71. 77.

of the heart and the diaphragm, depended upon the ſtructure or mechanical action of theſe organs. The learned author of the Zoonomia, who has adopted the idea of this connexion, has deduced it from that general law of the animal œconomy, by which motions that are frequently repeated in ſucceſſion, acquire the power of recurring in the ſame order, independently of the original exciting cauſe. Dr. Darwin obſerves, that "innumerable trains or tribes of other motions are "aſſociated with theſe muſcular motions which "are excited by irritation; as by the ſtimulus of "the blood in the right chamber of the heart, "the lungs are induced to expand themſelves."(a) But it may be remarked, in oppoſition to this ſuppoſed aſſociation, between the motions of the heart and the lungs, that in the fœtus the heart commences its contractions immediately upon its formation, while the lungs remain perfectly at reſt. When the animal leaves the uterus, the motion of the lungs commences, but the periods of the contraction of the diaphragm bear no determinate ratio to thoſe of the ſyſtole of the heart. The frequency of the pulſe is often increaſed or diminiſhed far beyond its accuſtomed ſtandard, while little alteration is produced in the motion of the diaphragm. The action of both theſe organs, however, depends upon the quantity and the quality of the blood tranſmitted to them, and any affection of this fluid, will of courſe influence both the circulation and the reſpiration. Theſe functions alſo both depend ultimately upon the contraction of the muſcular fibre, and are therefore equally liable to experience an alteration, from any circumſtance which affects the ſtate of the irritability or the ſenſibility of the ſyſtem. But it is obvious that theſe changes, though contemporary and proportional, do not depend upon aſſociation; they are produced by an aſſignable cauſe, operating directly upon each ſeparate organ, diſtinct from the effects of repetition or habit. Were it not foreign to my preſent ſubject, it would not, I think, be difficult to ſhew, that a ſimilar objection may be urged againſt many parts of the phyſiology and pathology of the Zoonomia, and particularly againſt the ſympathetic theory of fever.

(a) *Zoonomia, vol.* i. *p.* 46.

Mr. Hunter, ſuppoſes the exiſtence of this ſympathy, or aſſociation, between the motions of the heart and the lungs,*(a)* and the ſame opinion appears to be adopted by Dr. Currie.*(b)*

Note

(a) Hunter on the Blood, p. 54. *(b) Med. Reports p. 75.*

Note XXIV.

Helvetius, Senac, Winſlow, &c. Boerhaave, however, conceived that the lungs were rendered leſs capable of tranſmitting the blood during the ſtate of full inſpiration, and from the ſuppoſed effects of this retardation, he deduced his hypotheſis to account for the alternations of reſpiration.

Note XXV.

The celebrated experiment exhibited by Hooke, before the Royal Society, in which, after the motion of the heart had ceaſed, it was reproduced by inflating the lungs, has been adduced to prove, that by inſpiration the lungs are rendered pervious to the blood, *(a)* but it muſt be conſidered that in this caſe, the thorax was laid entirely open, and conſequently, when the lungs were not forcibly diſtended, they would collapſe ſo much as almoſt entirely to obliterate the cavities of the bronchia and veſicles.

Note XXVI.

Morgagni, relates a caſe, in which, upon examining

(a) Haller, viii. 4. 12.

amining after death, a conſiderable quantity of ſerous fluid was found in the cavity of the thorax, yet the pulſe had not been previouſly affected, nor had any impediment to the circulation been obſerved during the life of the patient.(a)

Note XXVII.

The experiments and obſervations of Mr. Coleman, upon the impermeable ſtate of the lungs in drowned animals, will more properly fall under our conſideration in a ſubſequent part of the eſſay.

Note XXVIII.

It has been obſerved, that when from any accidental cauſe, a portion of the cranium is removed, an alternate elevation and depreſſion of the brain is viſible to the eye, correſponding with expiration and inſpiration. This effect has been attributed to the reſiſtance which the blood experiences to its paſſage through the lungs in a ſtate of expiration ; a ſtagnation is thus brought about in the right ſide of the heart, and conſequently a leſs free diſcharge from the veins, on which account, in a highly vaſcular part, an increaſe of bulk will be produced; this is removed by the depletion of the veins, when the lungs from the enlargement of the thorax become again more completely pervious to the tranſmiſſion of the blood. There ſeems no reaſon to doubt that this phenomenon has been occaſionally obſerved, and it is probable that it depends upon the cauſe aſſigned above. But it muſt be remarked, that in thoſe inſtances, in which ſo great an injury is received by the cranium, the reſpiration is neceſſarily rendered laborious, the air is received into the lungs after long intervals, and conſequently in an increaſed quantity. *(a)*

(a) *Seats and causes of Diseases,* v. i. *p.* 408, 9.

Note XXIX.

It has been aſked, why are the lacteals furniſhed with valves, while the veins which paſs along the ſame viſcera, and are conveying their contents in the ſame direction, are not provided with them. One cauſe of this difference in ſtructure is obvious; the chyle, not being poſſeſſed of any vis a tergo when it enters the lacteals, is propelled along them ſolely by the contraction of their muſcular fibres, and the effect of this

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(a) Haller, vi. 4. 9; viii. 4. 11. 27.

to the period when this philoſopher entered upon his experiments, the name of Mayow is not mentioned.*(a)*

On the continent, although little eſtimated, and ſeldom referred to, his works were occaſionally quoted, and Haller in particular, certainly appears to have been acquainted with them. They were firſt again brought into notice in this country by Dr. Reinhold Forſter, who prefixed a ſhort account of Mayow's doctrines, to his tranſlation of Scheele's eſſay on air and fire.

The viciſſitudes of Mayow's reputation, afford a ſtriking inſtance of the tendency which exiſts in the human mind, to fly from one extreme to the oppoſite exceſs. When his works were thus brought before the public, his cauſe was eagerly embraced by different writers, his merits were exalted to the higheſt rank, and it was aſſerted, that he had anticipated the moſt valuable diſcoveries of Scheele, Prieſtley and Lavoiſier. *(b)* But a calm inveſtigation of the ſubject, will, I conceive, lead us to a very different concluſion. We ſhall,

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(a) Discourses, p. 1—20. *(b)* See particularly Dr. Beddoes's pamphlet, entitled, "Chemical experiments and "opinions, extracted from a work publiſhed in the laſt "century."

ſhall, indeed, find in this author many marks of real and original genius, and it is evident, that on ſome points, he extended his views beyond thoſe of any of his contemporaries. It muſt, however, be confeſſed, that the real addition which he made to the general ſtock of knowledge was not very great. He endeavoured to prove, that there was ſomething in the atmoſphere, which was an immediate agent in the proceſſes of combuſtion and reſpiration; but this had been long ago aſſerted by others, and when he comes afterwards to explain more in detail, his ideas reſpecting the intimate nature of this peculiar matter, he evidently ſhews that he had not any conception of the ſubſtance to which we now aſcribe theſe phenomena. He is even doubtful, whether it forms an eſſential part of the air, and upon the whole inclines to the negative ſide of the queſtion. In order to account for the alteration produced in the air by its removal, he adopts an hypotheſis to the laſt degree fanciful, according to which it conſiſts in ſome mechanical change in the configuration of its particles, which principally operates in diminiſhing its elaſticity. He imagines that this matter enters into the compoſition of the nitrous, and other acids, and that it gives them the peculiar properties which belong to this claſs of bodies, but he alſo ſuppoſes, that the ſame ſubſtance is a component part of the alcalis, and is the immediate cauſe of their cauſticity, and thus deciſively proves that he had no accurate conception of the modern doctrines reſpecting the conſtitution of acids.

As to the ſubject of reſpiration in particular, when ſpeaking of the action which the air exerciſes upon the blood, he has recourſe to the exploded doctrine of fermentation; he ſuppoſes that the particles of the air and of the blood, when brought into contact, are rubbed againſt each other, in conſequence of which, the former are deprived of that ſpirit to which they owe their elaſticity. I think that an attentive peruſal of Mayow's works will convince the diſpaſſionate reader, that there is no ſubſtance which has been diſcovered by the modern chemiſts, that can perform the various effects which are attributed by this author to his nitro-aereal ſpirit.

Perhaps the moſt important of Mayow's obſervations, are thoſe reſpecting the mechaniſm of reſpiration, and particularly the very decided manner in which he ſtates that the external and internal intercoſtals are equally ſubſervient to the enlargement of the thorax. The ingenious experiment, however, where the action of the lungs is illuſtrated by comparing them to a bladder incloſed in a pair of bellows, though brought forwards by Mayow as original, *(a)* is mentioned by Boyle in the moſt explicit manner, as what was already familiar to every one, and the concluſions which he deduces from it, are preciſely ſimilar to thoſe formed by Mayow. *(b)* There is one department, where the genius of Mayow was peculiarly ſuccefsful, by which, it is probable, he might have materially promoted the advancement of ſcience, had a greater degree of attention been beſtowed upon his writings; I refer to the contrivance of his apparatus and the execution of his experiments. In theſe reſpects he may be conſidered as far excelling any of his contemporaries, and as at leaſt equal to any chemiſt before the time of Scheele and Prieſtley.

In forming an eſtimate of the merits of Mayow, it is eſſential to obſerve, that a theory of combuſtion, almoſt preciſely ſimilar to that adopted by this philoſopher, had been a few years before publiſhed by Dr. Hooke, of which it is ſcarcely poſſible that Mayow could have been ignorant; it is however, not noticed by him. It appears, indeed, that Hooke has the advantage,

(a) *Tractatus quinque, p.* 274, 5. (b) *Works, vol.* i. *p.* 101.

tage, not only with reſpect to originality, but likewiſe in the accuracy and ſimplicity of his hypotheſis. According to Profeſſor Robiſon, Mayow's exhibition of Hooke's doctrine is, "obſcure, complicated, and wavering, mixed "with much mechanical nonſenſe, of wedges, "and darts, and motions, according to the "faſhion of the times. Hooke's conception of "the ſubject, is clear, ſimple and ſteady."*(a)* Dr. Thomſon paſſes a ſimilar judgment upon Mayow; he bears ample teſtimony to the originality and accuracy of his experiments, but he adds, that "his reaſoning is for the moſt part "abſurd, and the additions which he made to "the theory of Hooke are exceedingly extrava-"gant."*(b)* Dr. Yeats has treated largely upon the writings of Mayow, but has, in my opinion, very far over-rated their merit. He expreſsly attributes to him the diſcovery of oxygene, and ſuppoſes, that he was accurately acquainted with all its properties.*(c)* M. Fourcroy has publiſhed a candid and judicious account of Mayow in the *Enc. Method.* *(d)* which was afterwards inſerted in the *Annales de Chimie.* *(e)* In examining the writings

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(a) Black's Lectures by Robison, v. i. *p.* 535, 6. *(b) Thomson's Chemistry, v.* i. *p.* 347. *(c) Observations on the claims of the Moderns, &c. p.* 14, *& alibi.* *(d) Chimie, t.* iii. *p.* 390. *(e) t.* xxix.

ings of the older chemiſts and phyſiologiſts, it is neceſſary to remember, that expreſſions which appear to coincide remarkably with the modern diſcoveries, would not ſuggeſt the idea of theſe diſcoveries to one who was previouſly unacquainted with them. It has accordingly happened in ſeveral inſtances, that the modern doctrines have been ſuppoſed to be detected in books, where their exiſtence was not ſuſpected, until theſe doctrines had been deduced from the experiments and reaſonings of the chemiſts of the preſent period.

Note XXXI.

Dr. Black made this diſcovery by breathing through lime water, or the ſolution of a cauſtic alkali, when he found that the lime was precipitated, and the alkali rendered mild; effects which he attributed to fixed air emitted from the lungs; he conſequently conceived, that the change produced by reſpiration upon wholeſome air, conſiſts principally in the converſion of part of it into fixed air. It appears that he made theſe diſcoveries in the year 1757, and it may be inferred, that they were about the ſame time publicly announced in his lectures. *(a)*

(a) Black's Lectures, by Robison, v. ii. *p.* 87.

Note XXXII.

Beſides the oxygenous and azotic gaſſes, a ſmall quantity of carbonic acid gas, ſeldom exceeding ·01, has been found to exiſt in the atmoſphere, and is ſo conſtantly preſent in all the different ſituations in which the experiment has been tried, that it probably ought to be conſidered as one of its conſtituent parts. *(a)* The aqueous vapour, on the other hand, is perpetually varying in its proportion, and ſeems evidently to be an extraneous body, depending for its formation upon accidental cauſes. The proportion of oxygene which was indicated in the experiments of Scheele and Prieſtley, and in the more accurate examination of the atmoſphere, which was afterwards made by Lavoiſier, *(b)* is greater than that ſtated in the text. But the proceſs of Eudiometry has undergone of late conſiderable improvements, and the very accurate experiments of Berthollet, *(c)* De Marti, *(d)* and Davy, *(e)* agree in indicating the proportion of oxygene

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(a) Saussure, voyage dans les Alps, t. vii. *c.* 6. §. 2010. *Humbolt, Journal de Phys. t.* xxxxvii. *p.* 202. *Encyc. Meth. Chimie, Air, t.* i. *p.* 745. *(b) Elem. of Chem. chap.* 3. *p.* 86. *(c) Ann. de Chim. t.* xxxiv. *p.* 73 *& seq. (d) Journal de Phys. t.* lii. *p.* 173. *& seq. (e) Nicholson's Journ. v.* v. *p.* 175.

gene to azote to be nearly as 22 to 78. Theſe quantities muſt be conſidered as referring to the volume of the two gaſſes; their proportional weights are ſomewhat different, and will be as 26 to 74 nearly. *(a)* M. Fourcroy, however, in his late elaborate work, ſtill ſuppoſes, that the atmoſphere contains ·27 of oxygene. *(b)*

Note XXXIII.

I cannot avoid remarking, that in this memoir M. Lavoiſier ſpeaks of his obſervation reſpecting the carbonic acid gas, as of a fact diſcovered by himſelf, nor does he mention the name of Dr. Black, who it appears had publicly taught for twenty years that this aeriform fluid is diſengaged by reſpiration. It is much to be lamented that a man, who had ſo many real claims to the admiration of poſterity, ſhould in any inſtance have attempted to encreaſe his reputation at the expenſe of his contemporaries.

XXXIV.

This propoſition is ſubject to exceptions; M. Vauquelin found that ſome of the ſpecies of worms poſſeſſed

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(a) Thomson's Chem. v. iii. *p.* 269. *(b) Systeme des conn. chim. t.* i. *p.* 156.

poſſeſſed the power of ſeparating the oxygene from the azote in the moſt perfect manner. He performed his experiments upon the limax flavus and the helix pomatia.*(a)*

XXXV.

The two papers agree very nearly in the weight of oxygene ſuppoſed to be conſumed in twenty-four hours; the firſt makes it 19080 French grs. the latter 19090. But though the weights ſo nearly correſpond, the aſſigned volumes are conſiderably different; the firſt memoir ſtates it to be 24, and the latter ſomething more than 22 Paris cubic feet. If we ſuppoſe 100 cubic inches of oxygene to weigh 50·5 Paris grs.*(b)* 24 cubic feet, or 41472 cubic inches will weigh 20943·36 grs.; 22 cubic feet, or 38016 inches will weigh 19198·08 Paris grs. or 15750·33 Engliſh Troy grs.

The Paris weights and meaſures of the original are reduced to the Engliſh ſtandard by means of the formulæ given by Profeſſor Robiſon in Kerr's tranſlation of Lavoiſier's Elements, *App. No.* 5.

XXXVI.

It muſt be remarked, that at the time when Mr.

(a) Ann. de Chimie, t. xii. *p.* 278. *&* *seq.* *(b) Kirwan on Phlogiston, p.* 36.

Mr. Davy performed theſe experiments, he conceived the atmoſphere to contain ·27 parts of oxygene;*(a)* in the more accurate experiments which he afterwards performed, he found the proportion of oxygene to be little more than ·21.*(b)* This circumſtance will render it neceſſary to make ſome alterations in the eſtimate of the effects of reſpiration, as we may conclude that the ſame Eudiometrical proceſs was employed in the analyſis of air which had been reſpired, as of the air of the atmoſphere.

Mr. Davy's eſtimate.

100 cubic inches of atmoſpheric air before the experiment,

	Oxygene.	*Azote.*	*Carbonic Acid.*
Conſiſted of	27	73	
After the experiment of	22·5	71·7	4·5
Conſequently there was an abſorptfon of	4·5	1·3	
And a production of			4·5

Corrected eſtimate.

Before the experiment	21	78	1
After the experiment	17·5	76·6	4·5
Conſequently there was an abſorption of	3·5	1·4	
And a production of			3·5

Note

(a) Reſearches, p. 327. *(b) Nicholſon's Journal, v.* v. *p.* 175.

Note XXXVII.

Boyle firſt attempted to aſcertain the amount of the diminution, which the air experiences in reſpiration; he eſtimated it at $\frac{1}{30}$ part of its bulk. Mayow concluded from his experiments, that the air was diminiſhed $\frac{1}{14}$ part. *(a)* Hales made many experiments to aſcertain this point, but their reſults differ ſo conſiderably, as to render it impoſſible to fix any general ſtandard; it varied from $\frac{1}{30}$ to $\frac{1}{13}$ part of the whole air. *(b)* All theſe quantities, however different from each other, agree in being greater than the eſtimates of the modern chemiſts. This circumſtance is to be attributed to the abſorption of part of the carbonic acid formed in the lungs, by the water with which, in all the different experiments, the air of expiration was placed in contact.

Note XXXVIII.

The celebrated Sanctorius, who devoted ſo large a portion of his life to the inveſtigation of the quantity of matter perſpired from the body, under different circumſtances, eſtimated the pulmonary exhalation, at about half a pound in 24 hours.

(a) Tract. quinque, p. 405. *(b) Stat. Essays, v.* i. *p.* 238; *v.* ii. *p.* 320.

hours. (a) Dr. Hales, by purſuing the plan which is ſtated in the text, ſuppoſes the quantity to be above 20 oz. Dr. Home remarks, that the perſpiration from the lungs, is "computed "by Sanctorius to amount to 6 oz. in 24 hours, "by Hales to 23 oz.; the latter computation "ſeems too high, part of that moiſture being "probably owing to what is contained in a dry "air." (b) It appears, that in this inſtance, Dr. Home has been inaccurate in ſtating the calculation of Hales, and what is more remarkable, Haller, in giving the opinion of Home, ſays, "ad uncias 23 æſtimat Cl. Home," (c) and refers to the paſſage quoted above.

Note XXIX.

Dr. Prieſtley had previouſly obſerved this circumſtance, and had eſtimated that $\frac{1}{3}$ part only of the oxygene conſumed in reſpiration was employed in the formation of the carbonic acid emitted from the lungs; the reſt of the oxygene he conceived was abſorbed by the blood. (d)

Note XL.

It muſt be remembered, that a method of

(a) *Med. Statica, p.* 36. (b) *Medical facts, p.* 238. (c) *El. Phyſ.* viii. 5. 40. (d) *Experim. on Air, v.* iii. *p.* 378, 9.

eſtimating the quantity of water produced in the lungs, very ſimilar to that made uſe of by Lavoiſier, and founded nearly on the ſame train of reaſoning, had been previouſly employed by Dr. Crawford; he ſuppoſes that about $\frac{1}{6}$ part of the oxygene conſumed, enters into the formation of aqueous vapour. (*a*)

Note XLI.

This is not the only inſtance in which, after the concluſions of Dr. Prieſtley have been controverted by his contemporaries, a more accurate inveſtigation of the queſtion has ultimately decided in his favour. The complicated apparatus, and the impoſing air of minuteneſs, which characterize the operations of the French chemiſts, irreſiſtibly engage the aſſent of the reader, and ſcarcely permit him to examine the ſtability of the foundation upon which the ſtructure is erected. The ſimplicity of the proceſſes employed by Dr. Prieſtley, the apparent eaſe with which his experiments were performed, and the unaffected, converſational ſtile in which they are related, have, on the contrary, been miſtaken for the effects of haſte and inaccuracy. Something muſt alſo be aſcribed to the theoretical language which pervades

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(*a*) *Obſerv. on Animal heat, p.* 154, 347, 8.

vades and obſcures the chemical writings of this philoſopher, in conſequence of his unfortunate attachment to the doctrine of phlogiſton.

Note XLII.

Mr. Davy found that 71·7 cubic inches out of 99 which he expired were azote; (a) if therefore we eſtimate the average contents of the lungs at 280 cubic inches, 213 of theſe will conſiſt of azote.

Note XLIII.

In the firſt of the general concluſions, I have ſtated, that the atmoſpheric air received into the lungs, loſes about ·04 of the oxygene which was contained in it. Suppoſing the whole quantity of air reſpired in 24 hours to be 666·5 cubic feet, the quantity of oxygene which is removed will amount to 26·66 cubic feet. This eſtimate agrees exactly with the quantity of oxygene which I have ſuppoſed to be conſumed in 24 hours, deduced from the experiments of M. Lavoiſier and Mr. Davy. The quantity ·04 is however calculated from a different ſet of experiments,

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(a) *Researches, p. 432.*

riments, and the number 666 · 5 involves the eſtimate of a ſingle inſpiration, and of the quantity of air reſpired in 24 hours, both which were obtained from different ſources.

In the 2d general concluſion it is ſtated, that 22 cubic feet of carbonic acid gas are generated in 24 hours, ſo that there is, upon the whole, a loſs of 4 cubic feet in 24 hours, ſo far as reſpects the conſumption of oxygene, and the production of carbonic acid gas. In the 5th general concluſion it is ſtated, that a quantity of azote is conſumed in reſpiration, amounting to about $\frac{1}{160}$ part of the whole air employed. Suppoſing this, as in the former caſe, to be 666 · 5 cubic feet, the azote abſorbed, will be about 4 cubic feet. Theſe, added to the 4 cubic feet mentioned in the laſt ſentence, will make the total loſs by reſpiration in 24 hours 8 cubic feet. In the 3d general concluſion, it is ſaid, that the volume of the air reſpired is diminiſhed by $\frac{1}{80}$ part; now 666 · 5 divided by 80 exactly produces the number 8 · 33. The coincidence in this caſe is more remarkable than in the former, as it involves a greater number of quantities, all obtained by ſeparate calculations, and deduced from independent ſources. It muſt, I conceive, be not only regarded as a ſtrong argument in favour of their accuracy, but, in ſome meaſure, at leaſt, of the hypotheſes from which they were deduced.

Note XLIV.

M. Lavoiſier ſtates, that 100 parts of carbonic acid conſiſt of 72 parts oxygene, and 28 parts carbone,*(a)* but it has been diſcovered ſince the period of his experiments, that the charcoal which entered into the compoſition of the acid, is itſelf previouſly in the ſtate of an oxide, containing ·36 of oxygene; the compoſition of carbonic acid will therefore be pure carbone 18, and oxygene 82 parts; or the oxide of carbone which compoſes charcoal 28, and oxygene 72 parts.*(b)* Upon the ſuppoſition that the carbone in the blood is in the ſtate of the black oxide, there will in the ſpace of 24 hours, be a a ſurplus of oxygene of 5 · 59 oz.; but if the carbone be in a ſtate of purity, the ſuperabundant oxygene will be only 1 · 88 oz.

Note XLV.

This idea of the nature of the change produced

(a) Mem. Acad. 1789. *p.* 567. *(b) Thomson's Chemistry, v.* i. *p.* 50.

duced upon the blood by reſpiration, was founded upon a ſuppoſition, that its velocity was greater during its paſſage through the capillaries of the lungs, than in any other part of the circulation; an opinion which was at one time adopted by ſeveral phyſiologiſts of eminence. Dr. Hales, who performed experiments expreſsly upon this ſubject, informs us that he obſerved the blood to move five times as quickly through the lungs as through the muſcular capillaries.*(a)* Senac, on the other hand, thinks that its motion through the lungs cannot be quicker, becauſe each of the auricles muſt receive the ſame quantity of blood in the ſame time;*(b)* and others have ſuppoſed that the velocity muſt be leſs in the lungs, the pulmonary circulation being altogether ſhorter than that of the aortic ſyſtem, the velocity muſt be as the ſpaces gone through in the ſame time. Haller does not imagine that the average velocity in the lungs differs from that in the reſt of the body; he however remarks that it is ſent through them with leſs force; becauſe there are fewer obſtacles to its progreſs, and the right ventricle is conſequently leſs ſtrong than the left.*(c)*

(a) Statical Essays, v. ii. *p.* 66. *(b) Sur le Coeur, t.* ii. *p.* 161. *(c) Haller,* viii. 5. 21.

Note XLVI.

The following remarks of Boerhaave, who was diſtinguiſhed for his extenſive acquaintance with all the branches of ſcience, may ſerve as a proof of the entire ignorance reſpecting the change effected in the blood by reſpiration which prevailed in his time. He imagines that as long as the foramen ovale and ductus arterioſus are kept open, the animal can continue to live without air, becauſe the blood will then be able to paſs from the right to the left ſide of the heart. He even ſuppoſes, that by frequently plunging the young animal in water, the cloſing of theſe holes might be prevented, and the creature be thus rendered amphibious. Why we cannot dilate the cheſt under water, and continue to live in this ſituation, he ſays, " hoc nobis Κρυπτου eſt," but he adds with his accuſtomed candour, " quod forſan vobis, veſtroque ævo, aliquid pateſcet."*(a)* M. Buffon actually performed ſome experiments upon this ſubject.*(b)* Theſe opinions, which at preſent aſſume almoſt a ludicrous appearance, were pardonable at the period when they were advanced. But we can ſcarcely extend our apology to the phyſiologiſt, who after all the pneumatic diſcoveries which

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(a) Prælect. t. v. *pars* 2. *p.* 467—474. *(b) Nat. Hist. v.* ii *p.* 447.

which have immortalized the names of Black, Prieſtley, Lavoiſier, and Crawford, can ſtill fancy that, "by frequent immerſion in water, the aſ-"ſociation between the movements of the heart "and lungs might perhaps be diſſolved; and an "animal be inured to live commodiouſly, for any "time, under water." (a)

Note XLVII.

It is curious to obſerve the manner in which Haller ſpeaks of this experiment. "Hoc vulgare "experimentum non a Lowero ſolum, verum "etiam ab Helvetio ſerio propoſitum eſt."

Note XLVIII.

Dr. Crawford, in conformity with the theory at that time prevalent, imagined that carbonic acid was produced by the union of oxygene and hydrogene. He was confirmed in this error by the effects which hydrogene had been found to produce upon the craſſamentum in the experiments of Dr. Prieſtley, where bright ſcarlet blood, by expoſure to this gas, was rendered dark coloured, and acquired the venous character. Dr. Hamilton alſo found that the ſame change was produced in the colour of arterial blood by introducing hydrogenous gas into the carotid of a living animal. *(a)*

(a) *Beddoes on fact. airs, part* i. *p.* 41.

Note XLIX.

M. Haſſenfratz notices the objections which are mentioned in the text, yet he lays the moſt ſtreſs upon the manner in which the old theory accounts for the production of animal heat. He aſſerts, that according to it, the caloric is altogether evolved in the lungs, and that conſequently their temperature ought to be much ſuperior to that of the other parts of the ſyſtem. He ſays, that according to Dr. Crawford's hypotheſis, "les poumons ſont le foyer où ſe degage toute la "chaleur que le ſang abandonne dans l'economie "animale." And again, "M. de la Grange "reflechiſſant que si toute la chaleur qui ſe diſ-"tribue dans l'economie animale ſe degageoit "dans les poumons, &c." *(b)* When we conſider that this memoir was written many years after the publication of Dr. Crawford's treatiſe on

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(a) Crawford, p. 147, *&* *seq.* *(b) Ann. de Chim. t.* ix. *p.* 266.

on animal heat, it is ſomewhat ſurprizing, that ſuch expreſſions ſhould be employed by ſo experienced a philoſopher as M. Haſſenfratz. The friends of ſcience, muſt lament that the progreſs of knowledge is too frequently interrupted by a ſpirit of national jealouſy.

Note L.

Mr. Davy conceives, that after a complete reſpiration of atmoſpheric air, the gas contained in his bronchial veſſels, when reduced to the temperature of 55°, would amount only to 32 cubic inches, conſiſting of azote 23, oxygene 4·9, and carbonic acid 4·1 cubic inches.(a) He reſpired 100 cubic inches of the gaſeous oxide of azote for about half a minute, by this proceſs he found, that the gas was diminiſhed to 62 cubic inches, which upon examination, he found were compoſed of the nitrous oxide 29, azote 25·7, oxygene 4·1, and carbonic acid 3·2 cubic inches. He concludes that 32 cubic inches of gas, of the ſame compoſition, will be ſtill left in the lungs; theſe will conſiſt of nitrous oxide 14·7, azote 13·3, oxygene 2·1, and carbonic acid 1·9 inches. Before the experiment, the gas in the lungs, and in the apparatus, added together,

(a) *Researches, p.* 409.

together, were 132 cubic inches, compoſed of the oxide 100, azote 23, oxygene 4 . 9, and carbonic acid 4 · 1 inches; after the experiment, the gaſes in the lungs, and the apparatus taken together, amounted only to 94 inches, compoſed of the nitrous oxide 43 · 7, azote 39, oxygene 6 · 1, and carbonic acid 5 · 2 inches. From theſe data Mr. Davy inferred that 56 · 3 cubic inches of the oxide has been abſorbed, and 16 inches of azote produced. *(a)* Upon this experiment it may be remarked, that the 32 cubic inches ſuppoſed to be in the lungs at the commencement, is probably much leſs than the actual quantity which they contained. It is not expreſsly ſtated by Mr. Davy, that he made a complete expiration before the experiment, but granting this to be the caſe, we may conjecture, that above 100 cubic inches would be ſtill left in the lungs; now ſuppoſing the whole of this quantity to be of the ſame compoſition with the part examined by Mr. Davy, it will be compoſed of about 72 parts azote, 15 oxygene, and 13 carbonic acid; if to theſe we add 100 cubic inches of nitrous oxide, we ſhall have the whole amount of gaſes before the experiment. After the experiment, ſuppoſing that a complete expiration was made, there will ſtill be 100 inches left in the lungs; ſuppoſing with Mr. Davy, that the reſidual

(a) *Researches, p.* 412 *& seq.*

reſidual gas in the lungs, is of the ſame compoſition with that in the apparatus, theſe 100 inches will be compoſed of about 46 inches nitrous oxide, 41·5 azote, 6·6 oxygene, and 5·9 carbonic acid. If we add theſe quantities to the gaſes left in the apparatus, the whole amount of the reſidue after the experiment, will be nitrous oxide 75 inches, azote 67·2, oxygene 10·7 and carbonic acid 9·1 inches. Theſe numbers compared with the reſpective quantities at the commencement of the experiment, will give a deficiency in all the gaſes to the following amount; nitrous oxide 25 inches, azote 4·8 inches, oxygene 4·3, and carbonic acid 3·9 cubic inches. In making this calculation, it is aſſumed, that the 100 inches which are ſuppoſed to be in the lungs before and after the experiment, are of the ſame compoſition with the gas in the apparatus, but of this we have no direct proof. It is impoſſible therefore, to form any poſitive concluſions from an eſtimate founded upon ſuch uncertain data, but the reſult is unfavourable to the ſuppoſition, that azote is evolved in theſe circumſtances; perhaps if the experiment were repeated with the requiſite precautions, it would be found, that the only change which is effected, is an abſorption of part of the nitrous oxide.

Before the experiment, the gaſes were,

	Oxygene	*Azote*	*Carb. Acid*	*Nitr. ox.*	*Total*
In the lungs	15	72	13		100
In the appar.				100	100
Total	15	72	13	100	200
After the experiment,					
In the lungs	6·6	41·5	5·9	46	100
In the appar.	4·1	25·7	3·2	29	62
Total	10·7	67.2	9·1	75	162
loſt in the ex.	4·3	4·8	3·9	25	38

Note LI.

The facility with which the greateſt part of the oxygene of the atmoſphere is abſorbed in the various Eudiometrical proceſſes, and the difficulty of ſeparating the laſt portions of it, ſo as to obtain the azote in a perfect ſtate of purity, affords a ſtrong preſumption, that the component parts of the atmoſphere are united together in a ſtate of chemical compoſition, and not mechanically mixed, or merely diffuſed through each other's ſubſtance. It appears, that in this caſe, the atmoſphere is ſubjected to the ſame laws of affinity, which the learned M. Berthollet has ſhewn to exiſt in all chemical compounds. *(a)*

Note LII.

As the red particles can only be rendered viſible by the aid of the microſcope, the obſervations that have been made upon them, are deeply tinged with that uncertainty which always attends the uſe of high magnifying powers. In ſome inſtances we are able to point out, upon optical principles, the probable cauſe of the deception; but, in other caſes, it muſt be confeſſed, that there is reaſon to ſuſpect, that the obſerver ſaw what beſt coincided with his previous hypotheſis. A good account of the obſervations which have been made upon this ſubject, accompanied with ſome original experiments, may be found in the appendix to Mr. Cavallo's treatiſe on the factitious airs.

Note LIII.

In the Phil. Tranſ. for 1797, there is a paper on the colour of the blood, written by Dr. Wells, in which the author endeavours to overthrow the generally received opinions upon this ſubject. He conceives, that the alteration induced upon the colour of the blood, as well by the action of oxygene, as of ſome of the neutral ſalts, is altogether independent of any change effected by them upon its colouring matter. In order to prove this propoſition, he made a ſolution of the red particles (i. e. the colouring matter) in water, and he found, that the colour of this ſolution was not affected by the ſame reagents, which produce a change of colour in the craſſamentum of the blood. He alſo found that an aqueous ſolution of the red particles, whether taken from the craſſamentum of the arterial, or the venous blood, exhibited preciſely the ſame ſhade of colour. Hence he concludes, that the action of air, nitre, and other ſubſtances which redden the blood, is exerciſed not upon the red particles themſelves, but upon the medium which ſurrounds them. This action conſiſts in rendering the ſerum more denſe, and thus cauſing it to reflect a greater number of rays of light from its internal ſurface; and he attempted to prove by experiment, that the blood is in fact rendered more denſe, in thoſe caſes where its colour is brightened. The reaſoning of Dr. Wells is certainly ingenious, but, I think, not concluſive. We know, that by ſolution in water, the peculiar texture and organization of the red particles is deſtroyed, ſo that though no alteration of colour be produced by the air upon theſe bodies,

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(a) *Ann. de Chim. t.* xxxvi.

dies, when in this ſtate, it does not follow that the change would not be produced, if the air were applied to them while they retained their globular form. Our knowledge reſpecting the arrangement of the minute particles of bodies, is perhaps too imperfect for us to determine with certainty, by what means the changes of colour are effected, which follow from the application of different chemical reagents; an alteration in the denſity of the body, may probably be one among other methods; but ſuppoſing this to take place in the caſe now under conſideration, it is more natural to conceive, that the action ſhould be exercifed upon the particles themſelves, than upon the ſubſtance which is contiguous to them.

Dr. Wells, in the 2d part of his paper, offers ſome experiments to ſhew, that the red colour of the blood does not depend upon the iron contained in the red particles. They certainly deſerve conſideration, but if we admit them in their full extent, they would prove, that no part of the blood contains iron in the ſaline ſtate, a poſition which is directly oppoſed by the experiments that have been made more lately by Fourcroy and Vauquelin, who have not only confirmed the exiſtence of the iron, but have aſcertained, apparently with great exactneſs, the ſtate of combination in which it exiſts. The hypotheſis with which Dr. Wells concludes his paper, viz. that the red globules are compoſed of two diſtinct ſubſtances, poſſeſſed of different chemical properties, even though it coincides with the microſcopical obſervations of Mr. Hewſon, I cannot but conſider as altogether fanciful. Before we attempt to form any more conjectures on this ſubject, it would be deſirable to repeat and vary the experiments of Fourcroy and Vauquelin.

The imagination of the poet has in ſome degree anticipated the diſcovery of the chemiſts.

> When air's pure eſſence joins the vital flood,
> And with *phosphoric acid* dyes the blood,
> Your virgin trains the tranſient heat diſpart,
> And lead the ſoft combuſtion to the heart. &c.*(a)*

Dr. Darwin obſerves in a note, "Dr. Crawford, in his ingenious work on animal heat, has "endeavoured to prove, that during the combination of the pure part of the atmoſphere with "the phlogiſtic part of the blood, much of the "matter of heat is given out from the air; and "that this is the great and perpetual ſource of the "heat of animals; *to which we may add, that* "*the*

(a) Economy of Vegetation canto i. *l.* 399—402.

" *the phosphoric acid is probably produced by this* " *combination*; by which acid the colour of the " blood is changed in the lungs from a deep " crimſon to a bright ſcarlet."

Note LIV.

M. Abildgaard, of Copenhagen, made ſome experiments, of which a ſketch is given in the Annales de Chimie, (a) in order to determine the comparative quantity of carbone in the arterial and the venous blood. 100 Parts of the venous blood of a horſe, when dried by a moderate heat, afforded 26 parts of a dry pulverizable ſubſtance, whereas 100 parts of the arterial blood treated in the ſame manner, yielded only 25 parts. An ounce of venous blood, dried and decompoſed in a cloſe veſſel, produced 115½ grs. of carbonaceous matter; the ſame quantity of arterial blood produced no more than 87½ grs. Theſe experiments certainly appear favourable to the modern theory of reſpiration, but the account which is given of them is too conciſe to enable us to form any deciſive concluſions.

Note LV.

Dr. Bancroft firſt ſuggeſted the opinion, that charcoal was an oxide of carbone, *(a)* an opinion which has been ſince amply confirmed by the experiments of Guyton and Tennant. It is probable that this black oxide of carbone can only be formed by a degree of heat ſufficient to produce ignition, and conſequently much ſuperior to that of the blood. It may alſo be preſumed, from the whiteneſs of the chyle, that the carbone which it contains, is not combined with oxygene, or at leaſt, not in that ſtate of oxidation, which conſtitutes charcoal. Hence we may infer, that the inflammable matter of the blood exiſts in the ſtate of pure carbone, and conſequently, that the carbonic acid which is emitted from the lungs, is formed by the union of 18 parts of carbone, with 82 parts of oxygene. Dr. Bree ſuppoſes that the dark colour which the mucus of the bronchial glands ſometimes aſſumes, is owing to a portion of the carbone of the blood, which, in aſthmatic patients, is not carried off, in ſufficient quantity, by the formation of carbonic acid. *(b)*

(a) *T.* xxxvi. *p.* 91.

The opinion of Mr. Abernethy, reſpecting the origin of the carbonic acid diſcharged from the lungs, is very different from that generally aſſigned by the modern phyſiologiſts. He obſerves,

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(a) On the Phil. of Colours, p. 48. *note.* *(b) On Respiration, p.* 125.

that "at one time, phyſiologiſts believed that the "inſpired oxygenous gas, contributed to the production of the carbonic gas, found in the air "expired. This opinion perhaps ſtill prevails in "the minds of ſome people." (a) I conceive, however, that the doctrine which is here reprobated, is at preſent almoſt univerſally adopted. Mr. Abernethy ſuppoſes that the carbonic acid emitted from the lungs, is not properly the product of reſpiration; but is ſimply exhaled from the pulmonary veſſels. His chief objection to the contrary doctrine is, that the quantity of oxygene received into the blood by the lungs, is not ſufficient to generate the carbonic acid which is expelled. But on this point, Mr. Abernethy's opinion is in direct oppoſition to that of M. Lavoiſier, and the moſt eminent chemiſts, who conclude from their experiments, that the quantity of oxygene abſorbed, is conſiderably more than ſufficient to form the carbonic acid.

The opinion of M. Burdin, concerning the formation of the carbonic acid emitted from the lungs, is in ſome reſpects ſimilar to that embraced by Mr. Abernethy, and is in like manner very different from the one generally adopted. He imagines that all the ingredients of the acid exiſt in the venous blood, previous to its return to the lungs, and that when it arrives at this organ, they are united, and ſeparated from it by ſecretion. He ſuppoſes that oxygene is at the ſame time abſorbed by the blood, but that it has no immediate ſhare in the formation of the carbonic acid.(a) In oppoſition to this hypotheſis it may be urged, that the abſorption of oxygene and the production of carbonic acid, take place in the ſame manner, when a piece of craſſamentum is placed in a jar filled with oxygene, as when the venous blood is acted upon by the air contained in the veſicles. From this circumſtance it follows, that the effect produced depends altogether upon a chemical affinity between the oxygene, and one or more of the conſtituents of the blood, and not upon the operation of a function which can only ſubſiſt during life.

(a) *Essays*, *p*. 146.

Note LVI.

In forming this concluſion, I decide not only in oppoſition to the opinion of M. Lavoiſier, but of nearly all the phyſiologiſts who have treated upon this ſubject ſince the publication of his memoirs. The opinions of the French chemiſts, as far as I am acquainted with their works, almoſt

uniformly

(a) *Course of Med. Studies, v.* iii. *p.* 343.

uniformly coincide with that of their illuſtrious countryman.*(a)*

Among the Germans I find the ſame doctrine ſupported by Blumenbach, Jacquin, and Gren,*(b)* and in our own country by Crawford, Cavallo, Higgins, Currie, Thomſon, Rutherford and Allen. *(c)*

It is to be obſerved, that the formation of water is equally owing to reſpiration, whether in conformity with the opinion of M. Lavoiſier we ſuppoſe it to be immediately generated in the lungs, or in paſſing along the ſanguiferous ſyſtem, according to Haſſenfratz's hypotheſis. In both caſes it is produced by the union of oxygene abſorbed

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(a) Guyton-Morveau, Enc. Meth. Chimie, t. i. *p.* 727. 765. *Seguin, Ann. de Chim. t.* xxi. *p.* 225. *& Med. Eclaireé. Fourcroy, Systeme, &c. t.* x. *p.* 373. *t.* i. *p.* 56. *Hassenfratz, Ann. de Chim. t.* ix. *p.* 261. *Vauquelin, Ann. de Chim. t.* xii. *p.* 290. *De la Metherie, Journ. de Phys. t.* xlvi. *p.* 106. *Cuvier, Leçons d'Anat. comp. t.* i. *p.* 92. *Richerand, Phys. p.* 149.

(b) Blumenbach, Inst.Phys. p. 115. *Jacquin, El. of Chemistry,* § 956. *Gren, Ann. de Chim.* xxiv. *p.* 196.

(c) Crawford on Animal Heat, p. 154. *Cavallo's Elem. of Nat. Phil. v.* ii. *p.* 540, 1. *Higgins's Minutes of a Society, p.* 163. *Currie's Med. Reports, p.* 192. *Thomson's Chemistry, v.* iv. *p.* 497. *Rutherford in Marcet de Diabete, p.* 31. *Allen in De la Rive de Cal. Anim. p.* 35.

ſorbed by the blood in the lungs, and hydrogene previouſly exiſting in that fluid.

Note LVII.

M. Lavoiſier, in the two memoirs on reſpiration, written in conjunction with his friend Seguin, to which I have already ſo frequently referred, employs no arguments to prove the diſcharge of hydrogene from the blood in addition to thoſe ſtated above: as however theſe papers contain an account of the moſt elaborate experiments that have ever been performed upon the ſubject of reſpiration, and are the laſt works which this great philoſopher lived to complete, it may be deſirable to examine them with ſome degree of minuteneſs.

After ſtating ſome general propoſitions reſpecting the nature of combuſtion, and the compoſition of the bodies which are produced by reſpiration, they proceed to obſerve, that this function may be conſidered as a ſlow combuſtion of carbone and hydrogene, and that its effect is to abſtract from the blood a quantity of theſe ſubſtances, and to depoſit there a portion of caloric. There is, however, one circumſtance, in which reſpiration is materially different from combuſtion; this latter operation proceeds with more rapidity in proportion to the greater purity of the air employed, whereas the quantity of oxygene which an animal conſumes in reſpiration, is very nearly the ſame in whatever proportion it exiſts in the air received into the lungs. This propoſition was proved by a ſeries of experiments performed upon guinea-pigs. Theſe animals were confined for ſome days in air of different compoſition, from pure oxygene on the one hand, to air which only contained $\frac{1}{15}$ part of this ſubſtance. We are expreſſly told, that under theſe circumſtances, the reſpiration and circulation were not affected, and that the heat continued the ſame; in ſhort the only perceptible alteration in the ſtate of the animal was a degree of drowſineſs, when too large a proportion of azote had been employed. There appears no reaſon to doubt the accuracy of theſe experiments, the concluſions from them are probably different from what we ſhould have been led to expect; they however demonſtrate the abſolute inefficiency of the attempts which have been of late made with ſo much confidence to introduce at pleaſure into the ſyſtem, a greater or leſs quantity of oxygene, merely by varying the compoſition of the air preſented to the lungs. The authors ſtate, that during reſpiration there is neither any diſengagement nor abſorption of azote; they conſider this ſubſtance as entirely paſſive, and aſſert that any other gas mixed with the oxygene, which was not immediately noxious, would anſwer the ſame purpoſes with the air of the atmoſphere; they employed hydrogene, and found no inconvenience to ariſe from its uſe. It had been obſerved, that the quantity of oxygene conſumed in a given time, was different in different ſtates of the animal œconomy; the authors proceed to inveſtigate the amount of theſe changes, and for this purpoſe M. Seguin himſelf became the ſubject of the experiments. The circumſtances which they noticed as affecting the proceſs of reſpiration were, temperature, exerciſe, and the ſtate of digeſtion. As a ſtandard for the reſt of the experiments, they began by taking an average effect produced by a man at reſt, with the ſtomach empty, and at the temperature of 82°; under theſe circumſtances he conſumed in the ſpace of an hour 1210 cubic inches of oxygene. At the temperature of 57°, the other circumſtances remaining as before, he conſumed 1344 inches. During the proceſs of digeſtion, the conſumption of oxygene was increaſed to 18 or 1900 inches. While the ſtomach was empty, and the body in a ſtate of violent exerciſe, 3200 inches were conſumed in an hour, and when he uſed the ſame exertion after taking food, the quantity was increaſed to 4600 cubic inches in the ſame ſpace of time. Notwithſtanding this great difference in the quantity of oxygene conſumed, the temperature of the body was ſcarcely affected, but the circulation, and the reſpiration, were conſiderably quickened. The authors obſerve, that theſe numbers muſt only be conſidered as indicating proportions, becauſe it is probable, that individuals differ in the abſolute quantity of the changes which are effected by the reſpiration. The apparatus by which theſe experiments were performed, is not ſpecified. It muſt be obſerved that the effects produced upon the reſpiration by a change of temperature, had ſeveral years before been diſtinctly pointed out by Dr. Crawford, and M. Jurine had alſo aſcertained that muſcular exertion, and the ſtate of the ſtomach, produced very important effects upon this function, yet neither of theſe authors are noticed in the memoir now under conſideration.

Taking into account all theſe different circumſtances, the authors conclude, that about 1 cubic foot, or 1728 inches, may be conſidered as the average quantity of oxygene conſumed in an hour, and conſequently 24 cubic feet in the courſe of a day. They next proceed to point out what proportions of this oxygene are to be reſpectively aſſigned to the production of the carbonic acid, and the water. It is not expreſſly ſtated, in what manner the ſum of the carbonic acid was obtained, but this we may ſuppoſe to be ſufficiently accurate. It is then aſſumed, that this carbonic acid is compoſed of carbone, which previouſly exiſted in the blood, in the ſame ſtate of oxidation with charcoal, and the additional quantity of oxygene is eſtimated, which is required to form it into complete carbonic acid. The remainder of the oxygene is ſuppoſed to be employed in the production of water, and by calculating how much water can be formed by this ſuperabundant oxygene, the amount of the aqueous exhalation from the lungs is eſtimated. It will not be neceſſary to follow the authors through their details; I have endeavoured to ſhew that their calculations are built upon the aſſumption of data, one of which is improbable, and the other at leaſt incapable of proof. The memoir concludes with ſome general reflections deduced from the experiments. With reſpect to the increaſed conſumption of oxygene in a low temperature, the authors are inclined to attribute it to the greater denſity which cold air poſſeſſes, and to ſuppoſe, that the principal circumſtance which moderates the heat of the body in a high temperature, is the increaſe of tranſpiration, which carries of part of the caloric that had been previouſly evolved, by the formation of the water and carbonic acid.

Before I leave this memoir, I muſt remark, that in the eſtimate which is made of the quantity of water formed in the lungs, the figures which denote the weight of the hydrogene diſcharged, and of the water produced, do not correſpond. The whole quantity of oxygene ſuppoſed to be conſumed is 19080 grs.; the carbonic acid produced is 21600 grs.; this will contain 15553 grs. of oxygene. There will then be left for the formation of water 3528 grs., which will require 622 grs., or 1 oz. 46 grs. of hydrogene, and the quantity of water produced will be 4150 grs., or 7 oz. 1 dr. 46 grs. In the memoir under conſideration, the quantity of hydrogene is ſtated to be 1 oz. 5 drs. 51 grs. and the water only 5 drs. 51 grs.

The 2d memoir written by M M. Lavoiſier and Seguin, is expreſſly on the tranſpiration of animals. Tranſpiration is deſcribed as conſiſting in the loſs of a certain moiſture, which requires a quantity of heat to reduce it to vapour, in order to carry it off from the body; it proceeds both from the ſkin and the lungs, and according as it is derived from the one or the other of theſe ſources, it is ſtiled the cutaneous, or the pulmonary tranſpiration. Upon the greater or leſs quantity of this tranſpiration, together with the degree of denſity in the inſpired air, the authors conceive, that the uniformity of temperature depends, which the animal preſerves in all the different degrees of external heat to which it is expoſed. In addition to the aqueous vapour derived from the cutaneous and pulmonary tranſpiration, there is alſo the water ſuppoſed to be generated in the lungs from the immediate union of oxygene and hydrogene; it remained therefore to contrive a method by which the effect of theſe operations might be kept diſtinct and preciſely aſcertained; for this purpoſe the following apparatus was employed. A perſon was encloſed in a ſilk garment covered with a varniſh of elaſtic gum, he had a tube adapted to his mouth, and by this method it is ſtated, that the effects of tranſpiration and of reſpiration were kept diſtinct. But in this ſtatement there appears to be an inaccuracy; the effects of cutaneous tranſpiration would certainly be kept diſtinct from the effects of pulmonary tranſpiration and reſpiration, but it does not appear how theſe two were to be ſeparated.

By weighing the body juſt after entering the apparatus and juſt before leaving it, they endeavoured to aſcertain the loſs of weight which the body experiences by reſpiration, ſeparate from that by tranſpiration. By weighing the body immediately after entering the apparatus, and immediately after leaving it, the total loſs which it experiences by all the different ſources is aſcertained. The authors conjecture, that there exudes into the bronchia a humour which is ſeparated from the blood, compoſed chiefly of hydrogene and carbone, and which filtres through the membranes of the lungs. This humour, by being very much attenuated as it paſſes through the ſmall exhalants, is in part conſumed, and by decompoſing the oxygene with which it is in contact during inſpiration, water and carbonic acid gas are produced. The authors obſerve, that the carbonic acid is removed from the lungs by expiration, but that the water would be accumulated, were there not a method provided by nature to prevent this circumſtance. The air enters the lungs cold, but iſſues from them warmed nearly to the temperature of the blood, it will, therefore, diſſolve a part of the water, and remove it from the lungs. From this hypotheſis, which ſuppoſes that the combuſtible matter, diſengaged during reſpiration, is, in the firſt inſtance, tranſmitted through the pores of the lungs, in the form of an aqueous exhalation, it might be ſuppoſed quite ſuperfluous to aſſign any other ſource for the water of expiration; but it is immediately added, that the water exhaled is derived from two ſources, 1ſt. the water of pulmonary tranſpiration which we have been deſcribing, and 2d. that produced by the immediate union of oxygene and hydrogene, or what is ſtiled the water of reſpiration. We are informed, that in a future memoir the method will be detailed which was employed to ſeparate the water derived from theſe two ſources, and to aſcertain their reſpective amounts; but this memoir appears never to have been publiſhed, in conſequence, it may be preſumed, of the untimely fate of M. Lavoiſier.

The authors proceed to inform us, that by aſcertaining what quantity of water was ſent out of the lungs, and what quantity of carbonic acid was produced, and afterwards, by comparing theſe quantities with the quantity of oxygene conſumed, an eſtimate was formed of the water generated in the lungs, and of that which was evaporated from them. From the expreſſion employed there is reaſon to infer that the water exhaled was by ſome method, which is not mentioned in the memoir, actually collected and meaſured, and not as in the former inſtance ſimply calculated by a compariſon with the oxygene conſumed. But, ſuppoſing this to be accurately performed, before the concluſions could be obtained which are formed by Lavoiſier and Seguin, it would be neceſſary to prove that all the oxygene was employed in the formation of carbonic acid and water, and that the carbone had previouſly exiſted in the ſtate of charcoal. They ſeem to have been aware of the difficulty reſpecting the carbonic acid, and add, that if the carbonic acid be produced by digeſtion, the oxygene muſt be employed in the formation of the water of reſpiration, which will diminiſh the quantity ſuppoſed to be derived from the pulmonary tranſpiration; but it is ſtill taken for granted, that all the oxygene is employed in uniting with the combuſtible matter diſcharged from the blood. The mean reſults of the experiments are then detailed, and are as follows:—the loſs of weight experienced by the body in twenty-four hours is 2lb. 13oz.; this includes the loſs by reſpiration, and by both the cutaneous and the pulmonary tranſpiration; of this quantity the cutaneous tranſpiration takes away 1lb. 14oz., and the reſpiration 15oz.; it appears by a ſubſequent part of the calculation, that this eſtimate of the loſs by reſpiration includes alſo the loſs by the pulmonary

tranſpiration; it appears, indeed, that these two effects were neceſſarily confounded from the nature of the apparatus. The mean conſumption of oxygene in twenty-four hours is 2lb. 1 oz. 1dr. 10gr.; this oxygene is employed partly in the formation of carbonic acid, and partly in the formation of water. The weight of carbonic acid diſengaged in twenty-four hours is 1lb. 1 oz. 7dr. 4gr. conſiſting of oxygene . . 12oz. 0dr. 4gr.
carbone . . 5oz. 7dr. 0gr.

1lb. 1 oz. 7dr. 4gr.

There is then left a quantity of oxygene equal to 1lb. 4oz. 2dr. 10gr. which, in order to be formed into water, would require 3 oz. 3dr. 10grs. of hydrogene, which will produce a quantity of water equal to 1lb. 7oz. 5drs. 20grs.

The 2lbs. 13oz. which are ſuppoſed to be loſt in twenty-four hours, will be found then to conſiſt of the following quantities:

	oz.	*dr.*	*gr.*	*lb.*	*oz.*	*dr.*	*gr.*
Cutaneous tranſpiration				1	14	0	0
Carbone in the carb. acid	5	7	0				
Hydrogene in the water	3	3	10				
Pulmonary tranſpiration	5	5	62				
Loſs by reſpiration				0	15	0	0
				2	13	0	0

In this calculation it may be obſerved, that the two firſt quantities only are the direct reſult of experiment. The hydrogene was eſtimated from the oxygene left after the formation of the carbonic acid gas, and the pulmonary tranſpiration was a quantity aſſumed to ſupply the difference between the weight of the hydrogene and the carbone, and the total amount of the loſs by reſpiration. It has been already noticed, that the different experiments of Lavoiſier himſelf, indicate very different quantities of oxygene conſumed, and of carbonic acid produced in the proceſs of reſpiration.

Upon the whole, we may conclude, that theſe memoirs ſtill leave us much in doubt reſpecting the nature of the changes produced upon the blood by reſpiration, ſtill leſs can they be conſidered as aſcertaining with exactneſs the amount of theſe changes. At the ſame time we muſt acknowledge, that they exhibit an ingenuity in the contrivance, and a perſeverance in the execution of experiments which ſo juſtly entitle their illuſtrious author to the firſt rank among chemical philoſophers.

As my object in the analyſis of theſe papers was, rather to preſent a view of the method of reaſoning employed by Lavoiſier, than to enter into an examination of his reſults, I have retained the figures uſed in the original, which indicate the weights and meaſures of France before the revolution.

Note LVIII.

I have reſerved the diſcuſſion of the ſubject of animal heat to a ſubſequent part of the eſſay; I ſhall, however, in this place remark, that the doctrine which I have attempted to eſtabliſh concerning the non-emiſſion of hydrogene by the lungs, will materially affect our opinions reſpecting the influence of reſpiration upon the temperature of the living body. According to the generally received theory, the whole, or a part of the water which is diſcharged from the lungs, having been formed in the ſyſtem by the union of the gaſſes which form its component parts, produces the extrication of caloric in the ſame manner as during the generation of the carbonic acid gas. But if, on the other hand, we ſuppoſe that the water is generally received into the ſyſtem by the ſtomach, in the fluid ſtate, and is afterwards emitted from the body in the form of vapour, its diſcharge will be neceſſarily attended with a conſiderable expenditure of caloric. This removal of caloric from the body will take place in exact proportion to the quantity of water evaporated, either from the ſkin or the lungs, and conſequently we muſt conſider the diſcharge of aqueous vapour, either by the pulmonary or the cutaneous tranſpiration, as in all inſtances counteracting or moderating the effects produced by the diſcharge of carbone from the blood. According to this view of the function of reſpiration, the proceſs by which the body is cooled in high temperatures, is not confined to the operations of the ſkin, but is effected in an equal or probably greater degree by the lungs, ſo that we muſt regard this organ as poſſeſſing the power of imparting or abſtracting heat to and from the ſyſtem, according to the temperature of the ſurrounding medium, or the occaſional demands of the animal. The application of this hypotheſis to the operations of the living body, as well in the ſtate of health as of diſeaſe, is obviouſly of great extent and importance, but I ſhall not enter into any ſpeculations concerning it until I have endeavoured, by experiment, to eſtabliſh its validity. I ſhall only remark, that I think it will materially aſſiſt in the elucidation of thoſe anomalous facts in animal heat, which ſome eminent modern phyſiologiſts have conceived to be inexplicable upon the theory of Dr. Crawford. I allude particularly to the experiments and obſervations of the late Mr. Hunter, and to thoſe more recently made by my much reſpected friend, Dr. Currie.

Note LIX.

An hypotheſis ſimilar to this of M. Cuvier's, has been more lately propoſed by Dr. Thomſon;*(a)* it is advanced by him, as an original ſuggeſtion, and from the candor which pervades his writings, we muſt conclude, that he had not ſeen the work of M. Cuvier when he wrote the article of Animal Subſtances, in the *Supp.* to the *Enc. Brit.* The *Leçons d'Anatomie Comparée*, were publiſhed in 1800 (ann. 8), Dr. Thomſon's eſſay in 1801. M. Cuvier and Dr. Thomſon, in conformity with the prevailing opinion, conceive that both carbone and hydrogene are removed from the chyle when it is converted into blood, but we have no proof that the proportion of hydrogene is greater in chyle than in fibrine; the experiments that have been performed upon this ſubject only prove, that azote exiſts in greater proportion in blood than in chyle.

The hypotheſis which is adopted by M. Richerand, to account for the more complete animalization of the chyle, appears ſingularly perplexed. He ſuppoſes, that, as it paſſes through the lungs, after being mixed with the venous blood,

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(a) Chemistry, v. iv. *p.* 497.

blood, a great part of it is depoſited, "par une "ſorte de perſpiration interieure," into the parenchymatous ſubſtance of theſe viſcera. In this ſituation it is oxidated by the contact of the air, and being re-abſorbed by a number of inhalent veſſels, it is carried to the bronchial glands, where it depoſits part of its carbone, "et de fuligineux," and thus communicates to them their black colour. Being thus purified, it is returned into the thoracic duct, and poured again into the ſubclavian vein, whence it is brought back to the lungs, and again ſubjected to the action of the air. "De maniere qu'il ſe fait," ſays the author, "à travers ces organes, une veritable circulation "lymphatique, dont l'object eſt de donner au "chyle un degré d'animaliſation plus advancée."*(a)*

Note LX.

In ordinary experiments, oxygenous gas is obtained from a metallic oxide, heated in contact with the ſulphuric acid. But theſe oxides not unfrequently contain a quantity of carbonic acid, which will neceſſarily be expelled by this proceſs, and mixed with the oxygene; this is particularly the caſe with the red oxide of lead, a ſubſtance which frequently is made uſe of for the

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(a) Phys p. 160, 1.

the diſengagement of vital air. The black oxide of manganeſe, which is often employed for the ſame purpoſe, is not unfrequently mixed with the carbonate of lime, which will give out its carbonic acid during the proceſs *(a)* When the heat has been applied for a conſiderable length of time, and been in any degree violent, part of the ſulphuric acid itſelf will be converted into vapour, and be mixed with the gas. It is to accidental impregnations of this nature, that we muſt attribute the painful ſenſation which ſome perſons have experienced in attempting to reſpire oxygene.

Note LXI.

Some perſons have inferred from Dr. Prieſtley's experiments, that the reſpiration of oxygene is productive of injurious effects to the animal conſtitution. *(b)* This opinion was, I imagine, founded upon the obſervation of this philoſopher, that after an animal had expired in oxygenous gas, a 2d could continue to live for a ſhort time in the ſame air. Dr. Prieſtley, however, attributed the death of the firſt animal to the effect of cold, in conſequence

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(a) Cavallo on Fact. Airs, p. 203.

(b) Beddoes on Fact. Air, part i. *p.* 13.

ſequence of its paſſing through the water by which the gas was confined, and he accordingly was able to prolong the life of the animal for ſeveral hours, until the air was completely deoxidated, by placing it near the fire. "*This experiment*," he adds, "*fully satisfied me, that it was nothing in the dephlogisticated air itself, that was the reason that mice could not live in it.*"*(a)* The deficiency of external heat appears evidently to have had an important effect upon the life of the animal, but there is another circumſtance, which I am inclined to regard as a ſtill more immediate cauſe of death, viz. a quantity of carbonic acid gas, which would be generated, and be confined in the apparatus. A freſh and vigorous animal would be able for ſome time to bear with impunity a quantity of this gas, which had proved fatal to one whoſe powers were exhauſted by the long continued expoſure to it. In confirmation of this opinion, I may remark, that the very ſame circumſtance has been found by Cigna, Jurine, Morozzo, and others, to occur in the reſpiration of a limited quantity of atmoſpherical air.*(b)*

M. Lavoiſier

(a) Exper. on Air, v. ii. *p.* 165. *(b) Journal de Phys. t.* xxv. *p.* 105, 6. *Chaptal's Chemistry, v.* i. *p.* 131, 2. *Enc. Meth. Art. Med. t.* i. *p.* 496.

M. Lavoiſier, in ſome of his earlier experiments made the ſame obſervation, viz. that after the death of the firſt animal, the air was left in ſufficient purity, to ſupport for ſome time the life of a ſecond. The ſame explanation may be had recourſe to in theſe, as in the experiments of Dr. Prieſtley; indeed, as M. Lavoiſier confined his gas by mercury, there would be a greater proportion of carbonic acid mixed with the oxygene, than when water was employed, as this fluid would neceſſarily abſorb part of the acid. When animals have been confined in oxygene, and the carbonic acid has been carefully removed by a pure alcali, or by lime water, the animal has completely abſorbed the whole of the oxygene without any inconvenience. *(b)*

We may remark, that the experiments of Dr. Prieſtley, in which he found an animal confined in oxygenous gas, to ſuffer ſo much from the effects of cold, afford a ſtrong preſumptive argument againſt the increaſe of temperature, which ſome ſpeculative chemiſts have imagined, muſt be produced by the reſpiration of oxygene.

The

(a) Higgins's Minutes of a Society, p. 132.

The only other obſervations upon this ſubject, which I find in the writings of Dr. Prieſtley, are contained in the following paragraph. "From the greater ſtrength and vivacity of the "flame of a candle, in this pure air, it may be "conjectured, that it might be peculiarly ſalutary "to the lungs in certain morbid caſes, when the "common air would not be ſufficient to carry "off the phlogiſtic putrid effluvium faſt enough. "But, perhaps, we may alſo infer from theſe "experiments, that though pure dephlogiſticated "air might be very uſeful as a *medicine*, it "might not be ſo proper for us in the uſual "healthy ſtate of the body; for as a candle "burns out much faſter in dephlogiſticated than "in common air, ſo we might, as may be ſaid, "*live out too fast*, and the animal powers be "too ſoon exhauſted in this pure kind of air." *(a)* It can ſcarcely be conceived, that Dr. Beddoes has built his poſitive aſſertion on the mere ſpeculation of Dr. Prieſtley; a ſpeculation ingenious indeed, and, at the time when it was formed, plauſible, but which has been ſince controverted by the direct experiments of M. Lavoiſier.

Note LXII.

"On ſait que la combuſtion, toutes choſes égales

(a) Exper. on Air, v. ii. *p.* 168.

" egales d'ailleurs, eſt d'autant plus rapide, que
" l'air dans lequel s'opère, eſt plus pur. Ainſi, par
" example, il ſe conſomme dans un temps donné
" beaucoup plus de charbon ou de tout autre com-
" buſtible, dans l'air vital, que dans l'air de l'atmo-
" ſphère. On avoit toujours penſé, qu'il en etoit
" de même de la reſpiration ; qu'il devoit s'accé-
" lérer dans l'air vital, and qu' alors il devoit ſe dé-
" gager ſoit dans le poumon, ſoit dans le cours de
" la circulation, une plus grande quantité de calo-
" rique. *Mais l'experience a detruit toutes ces opi-*
" *nions qui n'etoient fondées que sur l'analogie.*
" Soit que les animaux reſpirent dans l'air vital pur,
" ſoit qu'ils reſpirent ce même air, melangé avec
" une proportion plus ou moins conſiderable de gaz
" azote, la quantité d'air vital qu'ils conſomment,
" eſt toujours la même, à de très-lègeres differences
" près. Il nous eſt arrivé pluſieurs fois, de tenir
" un cochon d'Inde pendant pluſieurs jours, ſoit
" dans l'air vital pur, ſoit dans un mélange de
" quinze parties, de gaz azote & d'une d'air vital,
" en entretenant conſtamment les mêmes propor-
" tions ; l'animal dans les deux cas eſt demeuré
" dans ſon état naturel ; ſa reſpiration & ſa circu-
" lation ne paroiſſoient pas ſenſiblement, ni acce-
" lerées, ni retardées ; ſa chaleur étoit égale, &
" il avoit ſeulement, lorſque la proportion de gaz

“ azote devenoit trop forte, un peu plus de diſpo-
“ ſition à l' aſſoupiſſement.” *(a)*

Note LXIII.

Soon after it was diſcovered that oxygenous gas poſſeſſes the appropriate power of ſupporting animal life, the idea was ſuggeſted of employing it for medicinal purpoſes. As it was known, that in phthiſis the lungs are much injured in their texture, and probably rendered incapable of performing their functions to the full extent, ſome of the French chemiſts conceived that the reſpiration of oxygene might prove advantageous in this diſeaſe, by permitting the ſyſtem to acquire its due proportion of this element with greater facility. The idea was at leaſt as plauſible as the generality of medical hypotheſes, and the ſubjects of the firſt experiments, in conſequence of the happy deluſion which ſo frequently attends this fatal complaint, were inſpired with ſtrong hopes of the ſucceſs of the new practice. *(b)*

Shortly after this period a ſpeculation of an
oppoſite

(a) Lavoisier, Mem. Acad. 1789. *p.* 573. *(b) Chaptal's Chemistry, v.* i. *p.* 138. *Jurine, Enc. Meth. art. Medecine, t.* i. *p.* 500. *Fourcroy, Ann. de Chim. t.* iv. *p.* 83. *& seq.*

oppoſite nature was formed in this country. It was imagined that the ſyſtem, when affected by phthiſis, had in ſome way acquired too large a proportion of oxygene in its compoſition, and it was conſequently concluded that benefit muſt be derived from the reſpiration of a gas which contained a ſmaller proportion of oxygene than the air of the atmoſphere. The objections which were urged againſt this hypotheſis were indeed numerous, and the defenders of it were unable to point out by what means the ſyſtem could acquire this ſuperabundant quantity of oxygene. But all oppoſition was for ſome time ſilenced by the apparent ſucceſs which attended the employment of the pneumatic medicines, and ſo fully perſuaded were the profeſſion at large of the advantage likely to reſult from their uſe, that a public ſubſcription was actually commenced, in order to form an inſtitution, for the purpoſe of curing conſumption by the employment of the different gaſes. But the infatuation has at length ſubſided, and the plan ſeems to be abandoned by the projector himſelf, who cannot be ſuppoſed to want either zeal or induſtry in the promotion of his favorite ſcheme.

Note LXIV.

Few perſons, I conceive, in the preſent day, will object to this expreſſion. The pertinacity with which the oppoſite opinion was maintained by the determined followers of the Brunonian doctrine, appears to me one of the moſt ſingular conteſts of theory againſt experience, which has occurred in the hiſtory of modern ſcience. Dr. Smith, in his valuable diſſertation "de actione "muſculari," ranks "vapores mephitici" among the chemical ſedatives.(a)

(a) p. 33.

END OF THE SECOND PART.

In collecting the materials for this work, I have frequently found great difficulty in obtaining the necessary information, in consequence of the imperfection or inaccuracy of references. In order to prevent the recurrence of this obstacle to any future inquirer into the subject of respiration, I have noted my references with as much accuracy as lay in my power; and to remove the difficulty which is sometimes produced by the employment of different editions of the same work, I have subjoined a list of the books and treatises to which I have referred, marking the particular editions that were employed.

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PREFACE.

DURING the courſe of my attendance upon the fever-wards of this town, I had been frequently led to notice the rapid changes of temperature which the body experiences in this diſeaſe, and I felt deſirous to aſcertain how far they could be reconciled to the modern doctrines of animal heat. For this purpoſe, I entered upon a courſe of experiments, reſpecting the chemical ſtate of the reſpiration in fever, and I afterwards determined to extend my inquiries to other ſtates of the body, either natural or morbid, in which it might be ſuppoſed that this function would be affected. As however I was aware that the ſubject was not altogether new, it was proper to begin by making myſelf acquainted with what had been previouſly done by others, not only on this particular topic, but reſpecting reſpiration in general. It is well known, that this part of phyſiology has derived the moſt important aſſiſtance from the modern diſcoveries in chemiſtry, and that it has alſo immediately engaged the attention of ſome of the moſt diſtinguiſhed among the philoſophers of the preſent period. Their labours have, for the moſt part, been peculiarly ſucceſsful, and by their means we have obtained an inſight into ſome parts of the animal œconomy, which were before concealed in impenetrable obſcurity. But the information that has been acquired, ſtill remains diſperſed through a great variety of publications, ſome of which are voluminous, and others not eaſily to be procured; and in conſequence of the rapid advances of chemical knowledge, it happens in a few inſtances, that the concluſions even of the moſt judicious phyſiologiſts are contradicted by ſubſequent experiments. I conceived therefore, that my firſt object would be to collect the beſt authenticated facts, and the moſt valuable opinions that had been advanced, and to arrange them in ſuch a manner, as to preſent a correct idea of the preſent ſtate of our knowledge.

The only connected view of the modern doctrines of reſpiration, which we have in our language, is contained in an article of the Supplement to the Encyclopædia Britannica, written by Dr. Thomſon; and a ſection in the ſyſtem of chemiſtry publiſhed by the ſame gentleman. I have demonſtrated my idea of the ability of this author by the frequent reference which I have made to his writings; but the nature of the works reſtricted him within very confined limits. It was not without a degree of ſurpriſe and regret, that I peruſed the account of reſpiration, in the ſyſtem of phyſiology which is compiled for the uſe of the firſt medical ſchool now in exiſtence, a new edition of which was publiſhed in the year 1791, under the immediate direction of a gentleman of talents and information, and under the patronage of a Univerſity, which ranked among its profeſſors the names of Monro, Black, and Rutherford. The lectures on the animal œconomy, which were delivered at Edinburgh by Mr. Allen, were indeed of ſuch a nature as to ſupply every deficiency; it is much to be lamented that they are now diſcontinued, and ſtill more, that there is no proſpect of their publication; had that been the caſe, the following pages would have been entirely ſuperceded.

Every one who is acquainted with the nature of phyſiological experiments upon the living body, may eaſily imagine, that as I advanced in my propoſed plan, many unforeſeen difficulties occurred, and many new ſubjects of inquiry preſented themſelves, ſo that the probable period at which I may expect to have them in a ſtate fitted to meet the public eye, ſeems ſcarcely leſs remote than at their commencement. Under theſe circumſtances, I have been induced to preſent to the public this eſſay in its preſent ſtate; ſhould any unforeſeen accident prevent the completion of my plan, I have ſome expectation, that this portion of it will not be without its uſe; it may at leaſt ſerve to remove ſome obſtacles from the path of the future inquirer, and may have an effect in directing the attention to one of the moſt intereſting departments of natural knowledge.

Liverpool,
Feb. 24th, 1804.

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